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An Experimental Study of Fatigue

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CONTENTS.

AN EXPERIMENTAL STUDY OF FATIGUE.

I. INTRODUCTION.

This paper will offer first a statement of the work that has been done in the particular field of mental fatigue. It will then attempt to show by certain modifications in the usual methods of experimentation how this subject of investigation can be materially enriched and simplified. The general method presented here has been used in other experimental work, but so far as our knowledge goes, has not been employed in any continuous mental tests, or to any extent in experiments planned to determine the changes in physiological processes incident to mental work. Few 'fatigue' experiments intended to obtain results by an objective record have planned definitely to check these results with a more or less complete introspective report of what went on during the process.

We shall leave a critical estimation of this combination method till we have shown its results, and proceed at once to a short statement of the work of some of the more prominent investigators in the field of intellectual activity and mental fatigue.

II. HISTORICAL.

Preliminary Remarks.—In writing an historical statement of the fatigue problem,[1] the selection of material for mention is peculiarly difficult. Fatigue, in its various meanings, is an

[1] The reader will understand that the following sketch is preliminary to the experimental portion of this paper and not a *review* of the entire field. As such it aims to summarize, not reviews of specific papers, but rather the phenomena found by investigators to accompany decrease in mental activity, giving in addition only such matter not immediately germane to this purpose as seems to be necessary for the clear understanding of the phenomena presented. From this point of view the historical part is intended to be more or less exhaustive and to present a fairly complete survey of the field at present experimentally covered.

universal phenomenon of life. To discuss the subject in its entirety simply means the consideration of every sort of vital activity as it approaches the ebb. Nor is it sufficient to stop there, since the consideration of this phase of activity instantly leads on to the problems involved in the phase that has preceded, the period from the beginning of any specific form of activity to the attainment of maximum speed or efficiency. Even then our task would be incomplete, for the great majority of writers, early and late, have made no clear distinction between a possible normal phase of fatigue and that which borders on, or is plainly within, the field of pathological phenomena.

Still, in the beginning, it is possible to use the more or less artificial division to which general biological science finds itself committed. For example, one may properly and without doing violence to the more specific aims of this paper, omit any extended reference to fatigue phenomena as they appear in the fields of zoölogy and comparative psychology; nor should we expect such a paper to be held responsible for a complete survey of investigations in the purely chemical and physiological phases of fatigue. Finally, anything more than a passing notice of those problems usually denominated muscular fatigue and glandular fatigue is omitted.

Muscle Fatigue.—In view of its historical importance, it seems advisable to begin with a short statement of the results of investigations in the field of muscle activity.

The main original source of information with respect to muscle activity has been the experimentation with the extirpated muscle begun in 1870 by the classic work of Kronecker (52). The field since then has been filled with innumerable workers and the principal facts with respect to the activity of the isolated muscle obtained. The later work of Mosso (66) on the muscles of man and the invention of the ergograph made possible a considerable field of investigation left untouched by the artificial conditions of the extirpation process. The work of Bergström (7) and Hall (38) has brought the experimental apparatus and criticism of the physiological conditions to their present high point of perfection.

The work curve has been in the focus of attention since the

laws laid down by Kronecker (52). His first law is a statement of this curve of activity.

The curve of fatigue of a muscle which contracts at regular intervals, and with equally strong induction shocks, is represented by a straight line.

The second law may be stated as follows:

The height of the contractions diminishes the more rapidly the more rapid is the rhythm in which they are produced, and vice versa.

The animal on which his work was done was the frog, and the muscle used, the gastrocnemius. In the later work that has been done certain other facts have come out with reference to the activity of this muscle. The asymptotic character of the latter part of the curve was mentioned by Kronecker. His explanation referred this slowing of the decrease in the height of the contractions to the independent fatiguing of the 'contractile' and 'elastic' properties of the muscle, the latter fatiguing more rapidly and hence making the first part of the curve steeper than the end portion. The straight line character of the major portion of the curve persists whether the muscle be loaded continuously or be allowed total relief from the weight during the relaxation period. Hall (38) found recently that if the conditions were what he terms 'physiological,' the muscle of man, using the ergograph, could work indefinitely without signs of fatigue. By 'physiological' he means that it shall be compelled to carry a load proportional to the actual strength of the muscle, an amount to be determined empirically, and that it be allowed to rest from the strain of the weight during the period of relaxation. In earlier experiments, the muscle carried the weight back during relaxation and was therefore under constant strain.

The introduction of rest periods lengthens the time of the fatiguing process. Further, the muscle works longer if massaged or irrigated with defibrinated blood or some salt solution. Irrigation during the activity of the muscle does not change the form of the curve, but lengthens the period of contractility materially. After complete exhaustion the muscle requires a rest ranging from thirty minutes to two hours before it can

again be stimulated to the maximum; in the case of the extirpated muscle this maximum cannot again be reached, but signs of renewed activity are present after rest. In the live frog, after seven days of rest a muscle thus exhausted does not fully recover histologically, masses of dead muscle tissue still remaining in the muscle.

The fatigue curve of warm-blooded animals is quite similar to that of cold-blooded ones. One important difference appears in the much slower single contractions in cold-blooded animals, i. e., the time of contraction is materially longer. Later experimentation shows that Kronecker and the earlier experimenters overlooked some important characteristics in their curves on the live animals (55). Both warm and cold-blooded animals show what is known as the 'Treppe,' which is a gradual rise in the height of the contractions for a short time after beginning work. This rise lasts sometimes as long as seven minutes in a muscle irrigated by the circulation of defibrinated blood or a salt solution. In some cases Mosso found this phenomenon in the muscle contractions of man, though a decrease or negative 'Treppe' was just as frequently present.

Hough (42) and Mosso both found that after the muscle of man had been fatigued to the extent that it would no longer respond to voluntary effort, it could still be made to contract under electrical stimulations. Hough reports that the opposite is also true, i. e., the muscle fatigued by the electric shock is still capable of responding to voluntary effort. He finds that in cases of mechanical stimulation, less work is done both in the height to which the weight is raised in the individual contractions and also in the number of contractions. Lombard (59) found that, if the muscle became fatigued under voluntary stimulation to such an extent that it no longer responded to the stimulus, the continuation of the effort, without altering the position of the muscle or otherwise aiding its recovery, would in time again produce contractions of the muscle. These contractions began after only one failure in some cases; and total exhaustion as indicated by this failure to raise the weight never lasted for more than a few attempts. When the volun-

tary contraction was alternated with one mechanically produced, the two fatigue curves were typically different and seemed independent of each other. The first showed the alternate exhaustion and recovery periods, while the second was the ordinary straight line curve.

After the first exhaustion, the extirpated muscle can be forced to do still further work by increasing the strength of the stimulus, by lightening the load, by massage, by irrigation, or simply by the introduction of a rest period. Pure water is also efficacious, but in less degree, as a circulating medium. By any of these methods, the result of the work never equals that of the earlier test. In living muscle, this is also substantially true. Mosso found that a rest of at least two hours was necessary before the total work of the first period could be duplicated. Voluntary straining prolongs the time needed for recovery, and nutritive and hygienic conditions exert important influences.

In the extirpated muscle, there is no experimental possibility of the muscle being renewed by anabolic changes other than the utilization of oxygen. Placing the contracting muscle in a vacuum shows, however, that the supply of oxygen is not a necessary factor. It was at first supposed that the muscle would continue to contract, if but one condition were observed, the removal of carbon dioxide. This has been shown to be only partly true, and three fatigue substances have been definitely isolated as final products of muscle activity. These three are sarcolactic acid, $C_3 H_6 O_3$, mono-potassium phosphate, $KH_2 PO_4$, and carbon dioxide, CO_2. The accumulation of any one of these in varying quantities will bring about the above described phenomena, and its injection into the perfectly rested muscle will produce all the results of the natural accumulation. It was by the use of these three substances that Lee (55) worked out with some degree of fullness the phenomenon of the 'Treppe.' He showed that the introduction of these substances in (a) minute quantities for a considerable length of time or (b) larger quantities for a shorter time, would produce not a lessening of the height of the contraction but an actual increase in the amount of work done. Presented to the working muscle in the reverse of these conditions (i. e., a minute

quantity for a short time or a larger quantity for a longer period) the actual fatigue effects were produced at once. He concludes that their first effect on the working muscle is to increase its irritability, or to reduce the inertia and length of the 'warming-up' process, while their cumulative effect, if not removed by the circulation, is to reduce muscle irritability.

Speaking from the purely physiological standpoint, we find that the working muscle liberates heat in quantities far beyond the amounts that it gives off in the resting condition; its proteid compounds break down into simpler ones, three final compounds of which have been isolated; there is a greater demand for oxygen; for a more rapid circulation of the blood; and the metabolic processes in general are more active.

In some states that are apparently within the borders of the pathological, the muscles may be in a continuous state of contraction without any of these phenomena being present. In certain diseases, as muscularis dystrophia progressiva, and in special types of neurasthenia, the physiological and mechanical phenomena are modified in specific ways. Breukink (15) finds that in the first disease the height of the contractions is very much lessened, while the actual number of contractions that the subject can make is practically equal to that of the healthy muscle. In the latter disease, it frequently occurs that the subject is able to make a few contractions that equal those of the normal condition or are even greater, but they soon trail off into the tetanized condition or cease altogether.

In this connection it may be well to state something of the work that is being done in the field of the toxins in their relation to the fatigue problem. Dessy (23), Grandi, and Battelli in some experiments that were made on the suprarenal capsules early succeeded in isolating a substance that they called adrenalin. This substance when injected into the blood of a highly fatigued animal produced in him all the phenomena of a rested animal. They suggested that as a result of such experiments they had shown that there is a secretion produced in the activity of these glands that is antitoxic in its effects and acts by removing the fatigue substances from the blood. So also Weichardt (102) has quite recently claimed to have isolated a toxin that

destroys the usual phenomena of fatigue. Adrenalin has been shown in recent experiments to have merely a tonic effect. It is no longer referred to by the physiologist as having any antitoxic effects of the type assumed by these earlier investigators.

The locus of fatigue.—What has seemed the most tenable theory for the array of facts that we have set forth? The stages of explanation have passed in their historical development from the simple statement that the muscle used up oxygen and liberated carbon dioxide through the various possibilities suggested by the blood circulation and is finally showing signs of a toxic explanation of the phenomena. The most widely accepted view at the present time seems to be a statement of the facts in chemical terms. That the fatigue substances so far isolated tend when injected into the rested muscle through the circulation to produce all the phenomena that can be produced under the ordinary methods, goes far to substantiate the truth of such a statement. In this respect, the work of Lee (55) has put the final touches to such a theory. On the side of the chemist himself, the theory is not so easily accepted. All he can tell us is that certain chemicals do produce contradictory effects when given in varying proportions; but where the actual transition takes place and its *modus operandi* is yet unknown. Those who would turn to the neurologist find that there is no definite assistance from that quarter.

Without going into a discussion of the various theories it will be seen at once that there still remain several important points not definitely settled. Regarding the theory of an antitoxin, Lee says it seems more probable that the function of the suprarenals is one of supplying a general tonus to the system than of manufacturing any specific toxin. The chemical theory of fatigue seems at present best suited to explain the more elementary facts of fatigue. The question of the transition from a stimulating to a depressing effect in the action of a fatigue substance is still one for the physiological chemist to labor over. In this particular place it has seemed appropriate to ask what is the neural mechanism that enables this change in the mere quantity of a substance to produce such opposed effects? A second large group of questions arises when we ask

what are adequate physiological stimuli for the nerves of the visceral and deep-lying parts.

Experimental efforts to find the locus of fatigue are quite contradictory. Santesson (78) finds that the muscle will still give very large contractions when the stimulus is applied directly to the substance of the muscle after it has apparently been entirely exhausted by similar stimulations through the nerves. On the basis of other experiments that show the high resistance to fatigue of the nerve substance itself, Santesson (79) concludes that his experiments show that the first indications of fatigue are due to the wearing out of the motor end-plates in the muscles. In some later experiments made to corroborate his first results he finds that with a change in the strength of the stimulating electric current the muscle is still active; this change, under certain conditions stated in the article, can be one of increase or one of decrease in the strength of the current,—Wedenski's (103) condition of parabiosis. He does not find that this new result interferes in any way with his earlier theoretical conclusions. Schenk (80), whose article led to the repetition of the work of Santesson, finds that the results are not conclusive and concludes from his own experiments that the nerves and nerve endings are still functionally active. What actually happens is the loss of conductivity in the muscle tissue directly around the motor end plaque, and when the muscle is stimulated directly it responds as usual because it is now *adequately* stimulated. Joteyko (47), in experiments to be mentioned later and in her own review of the experimental field, argues with Santesson for the location of decreased functional activity in the motor end plaque.[1]

[1] Beside the results indicated above Breukink (15) found that, in the curves of certain psychopathic patients where they had been fatigued by the ergographic test, a rest of two minutes enabled them to go on with the test and to make contractions that were as high and in some instances even higher than the first series of the test, a phenomenon that he never finds occurring in tests made on well persons. In chorea the curve, where obtainable at all, is very irregular, combining characteristics of the voluntary curve with many that belong to involuntary curves and are out of the control of the patient. It will be extremely interesting and enlightening when such tests can be made with more knowledge of the causes of such diseases and the relations that exist between the muscles and nerves.

Investigations to detect fatiguability of the nerves have resulted in but little of a positive character. The experiments on direction of the electric current in the rested and the fatigued nerve show clear indications of a reversal of polarity, and the work of Brodie and Halliburton presents some indications of the presence of CO_2 as a product of neural activity. Later work exhibits the chemical phenomena in still greater detail, but attempts no answer to our specific question. Recent unpublished work of Dr. W. Koch indicates in extreme cases of exhaustion slight changes in sulphur oxidation in the brain as a whole. Waller (101) claims to have found the production of CO_2 during mental activity. In general, however, the nerves may be considered as presenting a resistance to the effects of fatigue far above that of any of the organs involved.

The work on the nerve cell to determine its coefficient of fatiguability was begun by Hodge (40), and up to a recent date has been based principally on such histological investigations. Hodge found that the nerve cell actually showed definite structural changes due to its functional activity. Some of these were a decrease in the actul size of the cell, a loss of stainable substance, loss of other protoplasmic substances causing a vacuolated condition of the cell, and a ragged and more sharply outlined contour. In recovery, he found that for sensory cells in the cat, a period of recuperation varying from seven to twenty-four hours is necessary for the complete return of the cell to its former shape and structure. Bethe working on the motor cell of the dog found practically the same series of changes after a considerable period of enforced activity on the part of the dog. The later investigations of Wedenski (103) and others show in greater detail the changes that occur in the active nerve cell, but do not enlighten us with respect to the locus of fatigue. The cell body has been assigned by several physiologists, notably Sherrington (87), two functions, one a trophic function and the other the power to act through the summation of stimuli, leaving, therefore, the directional function for nerve impulses to the synapse.

The investigations of Sherrington (68) probably constitute the most brilliant series of experiments on the whole field of

reflexes and problems connected wtih the 'blocked' pathway in nervous conduction. Through a long series of tests, he brings out a number of facts that are important for the proper envisagement of the field of neural physiology and one or two points that are of special interest for the problem concerning us here, he initial locus of fatigue. He finds that in the 'spinal dog,' one with a complete experimental section of the cord in the cervical region, after a time, certain reflexes return, notably the scratching reflex; and that these can be elicited by proper cutaneous stimulation in the thoracic region and at certain other points. The portion of the experiment of special interest here is that after cutaneous stimulation for a time in one small area, the muscles active cease to contract. But if a second place of stimulation be selected but a slight distance removed from the first, the same reflex may be immediately elicited, showing that the muscle itself is not in an exhausted condition. From this and other experiments, ~~too long for detailing now~~, Sherrington (87) also eliminates the possibility of mere sensory fatigue and concludes that the primary origin of this phenomenon of 'negative induction,' as he terms it (so-called muscular 'fatigue'), is at the synapse. To this point in the pathway of the nervous impulse Sherrington assigns the function of 'blocking' the impulse,—through the accumulation of chemical products resulting from neurone activity,—and the further function of 'shunting' the impulse to other pathways.

Joteyko (47), in a series of experiments on the gastrocnemius of a decapitated frog, blocked the pathway to one of the muscles beyond the synapses and continued the stimulation of the other muscle till exhaustion; upon opening the pathway to the first muscle she elicited the reflex with no signs of 'fatigue.' She concludes with Santesson (78) that the primary locus of this type of fatigue is the motor end plates in the muscles. Woodworth (106), by a stimulation in the medullic region, was able to produce the same fatigue phenomena that Sherrington (87) reports with the possible entire elimination of the synaptic centers. So whatever we may think with regard to the facts offered, the locus of muscular fatigue, and equally that of mental fatigue, is not yet settled by experimental, physiological neurology.

These few scattered statements must close our review of the phenomena of fatigue as they are connected with the less properly mental functions. This much seemed necessary in order to show somewhat more clearly where the problems that will meet us as psychologists seem to lie; and how referring the locus and control of this 'negative induction' phenomenon and 'intellectual fatigue' to the peripheral end of the 'reflex arc' will not constitute a wide departure from previous theory. Sherrington, while presenting the theory of its central origin, still notes the close analogy between the phenomena obtained by himself on the 'spinal dog' and the results of Lombard's experiments where Lombard (59) shows the recurring recovery of the muscle without intervening rest periods. Though Lee (55) in his review of the fatigue situation reaches the general conclusion just stated, he finds it far otherwise with the questions of mental fatigue. He quotes from a number of prominent psychologists to the effect that 'feelings of effort' doubtless have their origin in the periphery, but can find no statements regarding the 'sensations of fatigue' and their nature.

We may now pass to work that suggests more specifically than the foregoing the possibility of a psychological treatment of fatigue. This work attacks the problem of fatigue from quite a different angle. Because of this its importance for historical purposes is great, although from the point of view of the whole it marks rather the beginning stages of the psychology of fatigue than a period of permanent contribution. Unfortunately for both physiology and psychology the early attempts in the two fields are quite distinct. In general, this holds true till we reach the work of Mosso and again after we leave him to enter the second period. The one exception to this is Kraepelin, who drew his inspiration from personal contact with Mosso's work.

Mental Fatigue and its Bodily Expression.—Mosso (66) first called attention to the possibility of measuring intellectual fatigue through its effects on muscular ability. His results have been severely criticised by later writers, though partially corroborated. The method he used has been refined until the major number of the criticisms have been met, but as yet no

definite correlations have been obtained. The work of Mosso is, however, the beginning of investigation when we come to seek for physiological and psychological correlations.

Using the ergograph, Mosso finds, with a moderate load, at the rate of one contraction per two seconds, that the human muscle with voluntary contractions gives a fatigue curve different from the straight line curve of the frog's muscle. Two types occur, one a convex curve, the other concave. By applying electrodes to the arm and causing artificial contraction a curve approximating a straight line is obtained.[1] These types seem to be due to individual differences in becoming conscious of the 'tired' feelings. In intellectual and emotional excitement and strain these types persist, but certain others appear with them. For example, in the subjects tested, those whose curves of fatigue were concave in form (i. e., rapid fatigue effects at first, then slower) apparently find their muscular energy much reduced by an hour's lecturing or by holding examinations. On the other hand, Professor Aducco, whose curve of fatigue was convex, invariably showed an increased muscular power immediately after lecturing and during any intellectual strain, with a tendency of the muscle-work curve to fall much below the normal an hour or so after the strain was past. From the experiment of Maggiora (60), the laws of exhaustion are found to be dependent upon the previously fatigued or rested condition of the muscles or the brain.

These are the specific results of the ergograph work mentioned here, the details of which are given in the articles mentioned. The remainder of Mosso's discussion on intellectual fatigue is given over to a summary of collected observations from men who have shown evidences of severe mental effort. Many general phenomena of mental fatigue are noted, practically all of which are stated in a more systematic form in Galton's article referred to below. (See p. 30.)

Thorndike (93) finds that there is no correlation between the ability of school children to make records on the dyna-

[1] By straight line curve, is meant that the line connecting the summits of the successive contractions is a straight line running downward, finally to meet the horizontal line of no contraction.

mometer and the amount of work they have done in the school room. Binet (9) finds the reverse to be the case, confirming in his work that of Clavière (12), who found, where the children were taken in three groups (one group that had been working at the severer mental tasks such as Geometry, Latin, Greek, etc.; a second on less severe mental work, such as reading in Literature, Geography, History, and a third whose members were allowed to laugh, play, sing and otherwise amuse themselves in ways that were not too physically fatiguing) that they showed marked differences in the dynamometric tests. The first group gave without exception lower records after than before the mental work; the second group showed practically a standstill, some going above and others below their previous record; while the third group gave a consistently regular increase in their dynamometer test. Schuyten (81) finds that the dynamometer gives no certain means of testing the fatigue of school children; they regularly give the better tests first, whether these tests be taken in the morning or afternoon. Smedley (89) considers the dynamometer fairly accurate in determining the intellectual endowment of school children. Dresslar (28) found in his tapping tests that there was a steady increase in each hour of work. He thinks that intellectual activity tends to increase muscular alertness, and 'fatigue' is wholly muscular and produced by the 'strain.'

Scripture (82) found that, where the subjects were required to press a special form of dynamometer a certain amount, then increase that amount twice, three and four times, a continuous series of such judgments generally gave increasing inaccuracy and increasing uncertainty. He says (p. 75):

> Using 'fatigue' to mean a decrease in functional activity as defined on p. 14 we can consider both progressive error and the mean variation in the preceding experiments to be phenomena of fatigue. Although the actual force exerted increased as the efforts were repeated, yet, since they were intended to be equal to the first ones (or to the preceding ones), they became steadily less accurate. This increasing inaccuracy of judgment is properly a phenomenon of fatigue. Likewise the increasing uncertainty, as measured by the mean variation, is a constantly recurring phenomenon of fatigue.

Apparently the work done in this field need be pursued no further. There seems to be a singular lack of penetrating analysis with respect to just what the investigators working with the dynamometer are expecting, and no thorough-going criticism of the results. The criticisms of Müller (67) on the methods of Mosso are overlooked in the above tests, and no investigator reports any regular control series of tests on adults for the determination of practice effects, inurement and the like, such as were carried on by Bolton (10) with the ergograph. It is also curious that the work of Lombard (58) has been overlooked in the evaluation of these experiments. The work so far is entirely too haphazard to admit of any real estimate.

Relation of mental fatigue to automatic processes.—The next field where fatigue phenomena in connection with intellectual activity have been sought lies in the circulatory and respiratory processes. Only the main features of the work in this direction can be mentioned.

The work of Gley (38) marks the beginning of definite attempts to find a correlation between the progress of mental work and changes in the continuous automatic processes. In 1881 he made his first series of investigations, which he himself reviews in a monograph published in Paris in 1903. The statement he makes regarding his difficulty in determining the conditions of the problem and in getting unequivocal results is interesting and indicates great care in the conduct of the experiments.

The apparatus used was the cardiograph tambour adjusted to write the curve of the pulse from the internal carotid, a pneumograph for any changes in respiration and a Marey cardiograph for the heart curve itself. The apparatus was adjusted so that the hands and body of the subject were as free as possible when books or other objects were used in the conduct of the experiment for the mental tests. The same movements were carried out as nearly as possible during the period of comparative rest that were used in the control test. For the mental test, thirty of the experiments were made by reading Renouvier's "Traité de psychologie rationale;" twelve of them were made while working out problems in geometry, and ten

of them while making a series of multiplications, e. g., 529 × 9, and similar problems written out beforehand. In one sense, he remarks, this last might be considered as a memory test.

Out of thirty tests whose results he gives, three showed no increase in the pulse rate, all the others showing an increase from one to six beats per minute; the period of work ranging from ten to sixty minutes in length. The longer periods of work were the reading tests. The amplitude of the curve of the single beat was much increased. This means that the arterial pressure is less, the walls of the arteries are under less tension as far as their own musculature is concerned. In the third place, the dicrotic changes of the pulse are accentuated and become double, a phenomenon that also indicates an increased elasticity of the artery due to the relaxed condition of its walls, and its consequent distention due to the influx of blood. This increase in amplitude and the change in the dicrotic are most prominent at the beginning of the work period and frequently drop back to the normal size in the rest periods, when the work is continued for fifteen minutes or longer.

Gley finds that the radial pulse shows precisely the opposite series of effects; there is, so far as he can determine, no change in the form of the curve as recorded by the direct beat of the heart; and he is unable to find any changes in the respiration curve.

He concludes that there is a 'dilatation vasculaire cerebrale primitive,' and the blood vessels of the head are filled by the pressure from without, caused by the vascular contraction of the peripheral blood vessels. Binet (9) does not find that the results of the experiments made by Gley and others support this view.

Binet (9) and Patrizi (70) find that the changes in radial pulse or in the pulse of the periphery in general are frequently caused by conditions that are not intellectual, and that they frequently occur independently of any change in the cerebral pulse. The conclusion that Mosso (66) first reached corresponded to the theory maintained by Gley, but was given up by him (66) in his later writings where he came to hold the view maintained by Binet, that the two vascular systems were independent.

Other results of this type of investigation relate to the differences that appear in short intense periods of work when compared with longer periods. Binet and Courtier found that in short periods of work, two minutes and less, the pulse rate increased rapidly and maintained this acceleration for some time after the close of the work. MacDougall (38) showed that this increase in the rate was not an immediate one but took place in a graded fashion to a point of maximum speed, then gradually decreased. Gley (38) in criticising these results, says that this change might be due to the emotional state of the first part of the period. In prolonged intellectual work the opposite is the case after the first acceleration. When two periods of considerable length are compared, one with comparative relaxation both of body and mind and the other with bodily inactivity alone, Vaschide (9) finds that the intellectual work tends to increase the normal slowing of the pulse. When the work is prolonged over a number of days the same result is noted, the average of the pulse rate on the rest days being 76.86, and on the work days 71.04.

Experiments by Binet and Vaschide on the changes in blood pressure show that the sphygmomanometer when used with a low pressure acts as a plethysmograph and with a certain high pressure, one equal to the constant pressure of the blood, acts as a manometer and registers the actual blood pressure. In using this last method they get results showing that in muscular and intellectual activity, there is an increase in the blood pressure, while the amplitude of the pulse as recorded by the plethysmograph remains unchanged. This result is obtained from the capillary circulation in the hand. With the low pressure apparatus the result is exactly the opposite, the amplitude is diminished very noticeably, indicating a vaso-constriction of the periphery.

Gley's conclusions, based on his own work and that of others, are as follows: Intellectual work tends to accelerate the heart but when long continued tends to sharpen the diurnal curve of decrease in rate; it augments the blood pressure in the peripheral arteries; and these peripheral changes cause a series of secondary changes in the pulse wave, as a sharpening in the

angles of the wave, an increase in the number of the dicrotisms and a change in amplitude, and a decrease if measured by the plethysmograph; he also finds that the primitive dilation in the brain is a true phenomenon, and is attended by an increase in the volume of blood in the brain. These statements are corroborated by the results of others with the exception of the implication involved in the relationship that Gley finds to exist between the peripheral constriction and the cerebral dilation. Mosso in his later work, and Binet also, finds that as far as his present investigations will allow him to go, there is no relationship of a cause and effect kind existing between these phenomena. The argument that there is no causal relationship existing between the increase of the volume of blood in the brain and the intellectual effort, Gley disposes of with the remark that it is a common phenomenon that glandular activity always shows this reverse phenomenon in the beginning, and that the volumetric increase comes after the beginning of the mental strain is no argument that it is not a condition of that activity (p. 92, *op. cit.*).

Binet (9) in his own work and in reviewing the work of others, including Gley, finds that the objection to Gley's conclusion regarding the relation between the peripheral contraction and the cerebral dilation is well founded, and offers the method of determining the change in blood pressure as the most satisfactory one for determining the physiological effects of mental activity.

Pillsbury (72) found that there were indications of a relation between the attention waves as determined by the time when a gray ring was and was not visible on the Masson disk, and the Traube-Hering waves. The work of Zoneff and Meumann (62) on attention and respiration has shown that there is a distinct relation existing between the inception of mental effort and irregularities in the respiration curve. In addition there is a tendency to increase the number of respirations per minute in extreme mental work and a tendency to slow the rate in the more automatic activities. The work of Angell (2) and his collaborators showed the close relation between attention changes and the respiration curve prior to any other distinct

recognition of the fact, and the later writers mentioned have corroborated their results.

The above authors find a fairly constant correlation existing between the pulse rate and the respiration curve. They accelerate together and show the major portion of their phenomena in complementary phases. There are some exceptions to this general rule, but when they occur they are regular in the individual showing the variation, i. e., if the pulse accelerates when the rate of respiration is slowed, this relation remains constant for the individual showing the phenomena. The later work shows that certain variations are probably due to the change from abdominal to thoracic breathing or the reverse, facts that were not brought out till the breathing curve was recorded by two pneumographs. The writers mentioned find that the number of respirations per minute increases and decreases with the changes of pulse rate mentioned above. The quickening shows itself in a shortening of the rest period after the expiration and by the decrease in the amplitude of the whole curve. This last is sometimes designated a change from deep to superficial breathing. Meumann finds that this last phenomenon is not in reality such a change, but is rather a change from the abdominal to the thoracic breathing or the reverse.

Notwithstanding the apparently distinct relationships that seem to exist between the varying degrees of mental activity and the physiological processes, there has appeared as yet no definite attempt to explain the relationships thus found to exist. We have taken considerable space to describe the results in this field because of this striking lack. It may be that some one will see the need for such an investigation and correlation and make a more ambitious attempt to test experimentally these changes and to explain them.

Summary.—The actual bearing on the fatigue problem of the investigations here summarized is slight. The assumption is made all through the series of experiments that the mental work has gone on with equal speed from the beginning, and with equal facility and freedom from errors. In the shorter periods of work measuring less than a minute, this is not so serious a defect, but in the longer periods of work some sort of

correlation between the introspective account of the ease of working and the actual objective record of work done seems to be absolutely necessary if there is to be any definite determination of relationship. The main result of the work in this field so far seems to the writer to be one of promise merely. That the promise is a large one is admitted, but that it has so far any definite fulfilment cannot be maintained. Reference is made in this statement to the psychological import of the investigations. The one point that does seem to stand out in its psychological bearings is the relation between effort of the mental type that is made in the beginning of a piece of work, and the changes that accompany this effort in the breathing and pulse acceleration. Mental effort is shown by such relations to be bound up with the physiological expenditure of energy and the possibility of a distinct quantitative correlation becomes promising.

Reaction Time Tests.—Before passing immediately to the next field, i. e., of correlation between mental fatigue and sensory control, we may stop a moment to consider one type of experiment that seems to lie on the border line between the pure physical effects of fatigue and any sensory effects it may have. This border line problem is the reaction-time experiment.

Exner (32) found in 1873 that practice caused a decrease in reaction time, the opposing influence he called fatigue (Ermüdung). The phenomena of fatigue are much less striking than those for practice. The length of the reaction time increased or decreased with an increase or decrease in the intensity of the stimulus. A series of experiments on a man 77 years old whose reaction time was much longer than the normal showed that his nerves and muscles conducted as readily as a younger individual's and that the difference was, apparently, in the brain and nerve centers.

The first definite appearance of a fatigue problem is in the work of Professor Cattell. This work was done principally in the Leipzig laboratory and appeared first in a series of papers in the *Philosophische Studien*, Bd. iii and iv. The main portions of these articles are translated and appear in *Mind*.

In the first of the articles, Cattell (19) in discussing the time

of cerebral operations and the conditions of the experiment refers to these latter as placing "the subject in an abnormal condition, especially as to fatigue, attention, and practice." In a paper reported in the same volume, he discusses at some length the effects of these three factors; special tests were made to determine their separate influence on the reaction time. Only the tests on fatigue will be mentioned here.

In thirty series of twenty-six reactions each, when the first of each in the thirty series were averaged together, the second in each and so on through the twenty-six, the first average was always the shortest, but the others show no regular increase in the times or 'increase in the mean variation.'

If, however, we take the averages given by Cattell and group them by fours we get the type of curve that Kraepelin (50) and his school call the 'work curve' in which fatigue is, according to their analysis, a prominent factor. The figures for these averages are for subject B, 143, 143, 147, 149, 153, 151, and 150 s, the last being the average of two reactions both of which are 150 s; for subject C, 148, 149 .75, 148, 150.5, 153.75, 147, and 147 s, the last two reactions 144 and 150 s. A more exhaustive series was then undertaken; 1950 reactions were made in a single day covering practically all the time from 8:30 a.m. to 11:00 p.m. in B's series and to 1:30 a.m. in C's series. The combinations of a series included five series of reactions, light, white surface, letter, association, and sound reactions; this combination was repeated six times. To quote:

> The first result to be noted from the table is the very slight effects of fatigue; in no case is the time lengthened more than a couple of hundredths of a second and the mean variation is but very little increased. We reach the very unexpected result[1] that the processes which are the most automatic (naming letters and C's simple reaction times) are the most affected by fatigue. The determinations made on the following day show that B had recovered from all fatigue. In the case of C, however, the brain substance concerned in the simple reaction seems to have been so far exhausted that his reaction time remained abnormally long for two days.

[1] We shall hope to show in the sequel of this paper that such a result is, instead of being unexpected, exactly what might be looked for in the light of later investigations and their correlation with modern neurological theory.

With respect to daily increase in practice, Friedrich's (35) results agree with those of Donder (26), Exner (32), Cattell (19), V. Kries (51) and Auerbach. He finds in addition a practice effect which shows itself in a single day's experiments; the middle series usually shows a reaction time less than either the first or the last ones. The reaction test shows practice in both the simple and choice types, the latter being much more susceptible. Since only six to eighteen reactions were taken at a time, fatigue had no appreciable effect. Freidrich does not seem to consider the final falling off a fatigue effect.

Staude (91) remarks that fatigue of attention causes the speedy displacement of the apperceived idea by any other as well as by its related elements. This conception comes out in connection with a statement of the Wundtian doctrine of apperception.

Trautscholdt (94) finds that fatigue effects are most prominent in the case of those persons whose reaction times are fastest, and conversely. Only three subjects were used. He remarks that in the reaction to spoken words, in addition to the above, an uncertainty and perplexity of mind is felt, and that the mean variation becomes greater.

Kraepelin (50) finds in the reaction tests, after the immediate effect of anæsthetics has worn off, that in several instances, for ether and chloroform, there remains a feeling of fatigue that depends for its degree on the depth of the narcosis produced. With doses of alcohol up to 30 g. for those who are not regular users of it, a slight feeling of ease and an aroused condition appears, followed by a moderate feeling of fatigue. In the case of larger doses, up to 60 g., after six or eight minutes a feeling of intoxication appeared which gave rise to greater certainty, and greater clearness of perception, in reacting. This favorable influence gave way in the course of half an hour to an uncomfortable feeling of fatigue and stupidness and actual sleepiness. The effect of narcotics upon muscular and mental activity is at present an important field of experimentation. Rivers is perhaps the most active of recent investigators (76a).

When the reaction experiment is carried on continuously so

that fatigue effects are allowed to accumulate, we have the fatigue experiment in its proper form once more. Two experiments of this sort are recorded, one by Patrizi and the other by Scripture. Patrizi (70) used the ordinary methods of determining the reaction time, with the exception that he allowed only intervals of two seconds to intervene between the reactions. He found a lengthening of the average time and an increase in the mean variation.

Scripture (82) in his experiments attempted to determine the rate at which the different factors used in the reaction fatigued. To do this the first series was made up of reactions to small flashes of light, where the observer sat in a dark room with both eyes open and relaxed. A second series was used with conditions as before except that one eye was bandaged to reduce convergence, while a third series was carried out with the subject looking constantly at the tube, one eye still bandaged, so that accommodation was a steady process. In the last test the room was lighted. Attention was present in all three cases, but the factors, as far as the eye was concerned, were considerably modified.

In the first record, after five minutes it was found that the reaction time had increased 60 per cent, and the time of holding down the key over 130 per cent, while the irregularity as shown by the mean variation was four times as great. With one eye bandaged, the increase in all three of these changes was distinctly less, while with one eye covered and a light in the room the change due to fatigue was scarcely apparent; there was an increase in the reaction time in the first five minutes of less than 5 per cent, an actual decrease in the mean variation, and only a six per cent increase in the period of inertia that preceded the reaction. The following conclusion may be quoted:

> The fatigue in reaction time increases with the complexity of the adjustments required for perceiving the stimulus. There is least fatigue when only an effort of attention is involved, more when the act of accommodation is added, and still more when the act of convergence is also added (p. 19).

Burnstein (5) found that there was a loss in the speed of the

reaction time due to a 'general fatigue' from a day's work, of $15\frac{1}{2}$ per cent in the simple reaction and of 17 per cent in the complex reactions. The morning record was taken at 8:30, and the afternoon record at 5:30. We may close this portion of the review by a quotation from Scripture's (82) article:

> There are three different phenomena denoted by the term 'fatigue,' (1) a chemical change in the constituent parts of the organism, (2) a diminution in functional activity, (3) a group of sensations.

Special and Cutaneous Senses.—We now pass to the more definitely sensory field where attempts have been made to determine the effects of mental work. We find an almost total lack of literature and attempts at experimentation, till we come to the subject of cutaneous sensibility. The special senses other than those of touch and the general cutaneous field are not, apparently, correlated with mental activity in the sense that the term 'fatigue effect' implies. As a result, there appear no definite tests on this topic. To be sure, numerous observations are recorded regarding the effect of continuous activity of one sense organ on its own power of reacting thereafter, and on the changes in fineness of discrimination. Results are especially clear in the realm of taste and smell, where the separate end-organs fatigue with considerable rapidity. In sight and hearing the fatigue effects arising in the epithelial end-organ itself do not appear so clearly. The fluctuations of attention when the sound or light observed are near the threshold of sight and audition, constitute in the minds of some observers a valid fatigue phenomenon; other investigators explain these changes in other ways. There is some tendency to call this 'fatigue of attention.'

Some isolated passages in this connection may be quoted, though no attempt need be made to evaluate them or find a relation to the specific field. The references are too sporadic and uncontrolled in most instances to constitute any more than naïve observations.

Berger (4) states that in his experiments to determine the variations in reaction times for stimuli of varying strengths, only attention of ordinary habitual concentration was used and the

effects of practice and fatigue are not noticeable. In fact, they are reduced to such an extent that their effect on the reactions cannot be determined. He adds that the changes in attention are not the regular, steady increase or decrease that practice and fatigue ought to show.

Wolfe[1] finds that we have a species of tone memory or immediate recall in the case of tones, or, as Fechner stated it, an ability to retain after-images. This shows variations in degrees of clearness, and Wolfe concludes that these variations are due to changes in attention, even when the same amount of effort is put forth. This is fatigue of the organ of apperception (Apperceptionsorgan). This is, however, merely a suggestion. "A probable phenomenon of fluctuations of attention" is that "the periods will be shorter the simpler the object, and the greater the effort put forth." The actual fatigue as shown by the numerical variations of the two-hour tests in nowise corresponds to the feeling of fatigue. This, Wolfe remarks, may be due to the development of a temporary readiness to work (vorübergehende Fertigkeit) or skill (Einübung), which masks the real fatigue.

Lange[2] also refuses to call these fluctuations (when they occur during the continuation of the stimulus) instances of sensory fatigue, but assigns them to central causes, changes in the condition of the attention as it is directed to the stimulus (sinnliche Aufmerksamkeit). Münsterberg assigns them, however, to sensory sources.[3]

So far, therefore, the problems connected with the fluctuations of attention have not been brought into the realm of measurable fatigue, except as foot-notes and precautionary directions. The reaction time may with propriety be considered as connected with the sensory field; but in general the emphasis in the search for fatigue effects has been confined to the motor process. Beyond the work of Scripture (82) referred to above, and the work of Angell and Moore, (2) the

[1] *Philos. Stud.*, v. 3, 560 ff.
[2] *Philos. Stud.*, v. 4, p. 390 ff.
[3] Münsterberg, H.: *Beiträge zur Experimentellen Psychologie*, Heft 1, II, p. 69 f.

writer knows of no attempt to assign proper fatigue values to the two phases of the sensory motor process. Baldwin's (3) work is merely a pioneer article on the analysis of these processes. Scripture says that in the first ten minutes of the continuous form of the reaction test the attention process shows little signs of fatigue. Angell and Moore find that attention goes with the part of the process that is least habitual. Where the subject shows considerable facility in one form of reaction, to direct him to turn his attention to the other portions of the process causes a rise in the reaction time and an increase in mean variation. Fatigue will also appear sooner when the subject is working under these new conditions than when working under the more habitual ones. Voss (99) found that in continuous additions there appeared a wave of effort which correlates quite easily with the attention waves found by Hylan and other investigators. We shall mention his work more in detail later.

We must omit for the present any more specific reference to the work done on the changes in attention. It will again be referred to when the effects of mental work are discussed in connection with the experimental results of this paper. Once the fatigue phenomena are assigned to their proper place in memory, attention, perception, judgment, etc., we may then hope for a discussion of fatigue that will be able to emphasize the psychical side more than is at present possible.

Cutaneous Changes.—We have mentioned above that there is one field in which a definite correlation has been offered between sensory discriminations and purely mental fatigue, or the effects of mental work. The power to discriminate two points on the skin had previously received considerable experimental notice, but it remained for the staid German investigators to find that herein lay the means for preventing scholastic overwork.

In this field of cutaneous sensibility the work has been most completely carried out by Griesbach (38–1), Wagner (100), and Germann (36–1). Griesbach was the first in the field and found a distinct correlation between the work of the school room and the fineness of sensory discrimination on the

skin. The principal test was the two-point discrimination test. Wagner and Vannod (97) also found a distinct relationship existing between the power to discriminate two points placed on the hand and mental fatigue. This capacity in their experiments is in inverse ratio to the amount of intellectual work done, being in the subjects tested fairly constant when the day was spent in resting or on holidays, but decreasing in fineness quite markedly on school days. They found that there was a definite ratio between fineness of discrimination and the kind of subject matter studied. According to their experiments, one subject of the school curriculum is decidedly different in its fatigue effects from its neighbor.

R. MacDougall (63) in a review of several articles on fatigue reports as follows on the work of Wagner and Griesbach. The method of Wagner is described.

> An area was selected upon the cheek and jaw of which the normal discrimination distance for two touch impressions was taken before school work began. The amount of fatigue was measured by the distance in discrimination acuteness which appeared after each successive hour of school work. This form of test has apparently proved decidedly satisfactory; it unites the simplicity of the physical method with a direct psychological fatigue factor. . . . The facts cited point to the conclusion that mental work of every kind is accompanied by general and not localized central fatigue, and it is questionable if new faculties can be appealed to. . . . All mental work involves fatigue of all and every part of the pupil's faculties, and there appears no absolute escape from it by variation of studies.

Schuyten (81) after testing all previous methods of determining the degrees of fatigue in school children finds the æsthesiometric test most reliable.

In Germany, some opposition developed toward this theory, particularly through experimentation done in the laboratory at Heidelberg. Bolton (10) working with Kraepelin found that a large number of factors arose in taking determinations as rapidly as these men did, all of which in his own experiments vitiated the results. The rise and continuance of after-images and tingling sensations were the more prominent of these factors. The relation of one portion of the skin to another immediately adjacent also prevented accuracy in the determinations.

Tümpel, after reviewing the work of Griesbach and Vannod, for theoretical reasons, finds their work wholly unscientific and unpsychological. Ritter (75), also for theoretical reasons, finds their results untenable.

Binet (9) and his collaborators in the schools of France find results that stand in close correspondence with the work of Griesbach. Using the æsthesiometer and algometer, distinct fatigue effects were found present in children of all ages after the work of the school day. These showed in the decreased ability to discriminate between two points and an increased sensitivity to the pressure of the algometer. Binet concludes by saying that the work of the psychologist is done and all that remains is the application of the method by the pedagogue.

The major portion of the work, however, that is hostile to the correlation found by these investigators has been done in America. Of this, the work of Leuba and Germann seems most conclusive. Leuba (56) went over the experiments of Griesbach and followed his method as closely as it could be determined from the articles published. He found there was no such ratio as Griesbach had found, in fact the opposite ratio was as frequently the case. He shows from the figures that Wagner published that the agreement between Wagner's and Griesbach's results is not nearly so perfect as Wagner had thought. Griesbach's results, so Leuba reports, show a rather regular and continuous decrease in sensibility lasting throughout the first hour, while Wagner's results show the decrease only at the end of the first hour. Leuba's criticisms on the methods used by these two experimenters are as follows: The rapidity with which the tests were made, six to ten persons in one area in ten minutes, or two or three persons in six different places in the same time, constitutes a feat that he considers as verging on the impossible; no preliminary tests were given; and in the tests made by Wagner, no precautions were taken to prevent errors due to the changes in temperature of the skin on coming into a warm room; the tests were taken in February and March.

Germann (36–1) in a series of carefully conducted tests on a single individual found that the number of errors in an equal

number of morning and evening tests averaged 15.1 per cent ± 6.8 per cent in the morning series, and 12.6 per cent ± 9.1 per cent in the evening series; the percentage is based on the whole number of tests. He suggests as his conclusion regarding the validity of the methods employed by Griesbach, Vannod and Wagner, that the normal fluctuations found by Judd and Tawney (92)

> correspond quite closely with the amplitude of variation which Griesbach and more recently Wagner, ascribe to the influence of fatigue.

In regard to his own experiments he says:

> In at least one normal case the percentage of errors in cutaneous tactile discrimination bears no constant nor even relative correspondence to the mental fatigue experienced by the subject. I am convinced that in special cases the æsthesiometric method is absolutely inadequate for the determination of mental fatigue. Moreover, I strongly doubt its validity in any case.

From these results, one must still entertain strong doubts regarding the validity of the correlation found in a short series of experiments whose technical accuracy is so much in doubt. We are therefore left in uncertainty with respect to this attempt to correlate sensory acuteness of discrimination and mental fatigue. On the other hand, seeming to point to the possibility of such a relation, we find Kraepelin and his school insisting on a definite and positive correlation between the susceptibility to fatigue and its actual rise, and the capability of 'practice gain.' This last power is, by most investigators, definitely related to the sensory motor-arc and the establishment of a habitual pathway. But the preponderance of the evidence so far points to no such sensory specialization in the rise of fatigue, either as a 'diminution in functional activity,' or as 'a group of sensations.'

The work of Bettman (8) and others in determining the effect of one type of mental work on another uses functions too complex, adding, reading, memorizing, etc., to enable us to make a judgment on this point. Thorndike (93) found that fatigue was not transferable. On the other hand, practice is repeatedly found extending its influence beyond the specific stimulus-

and-response pathway actually used.[1] 'Negative' practice, or interference of training, may later be found to be definitely related to fatigue phenomena.

If the writer's personal reaction may be allowed at this point, it seems possible that under certain conditions a very distinct relationship may be found existing between cutaneous anæsthesia and analgesia on the one hand and specific forms of mental activity on the other. We believe, however, that experimental methods are not yet sufficiently developed to make this correlation.

Changes in the mental experience during the mental activity.— The approach to the final group of observations is fraught with considerable difficulty. There seems to be little or no continuity existing among experiments made to determine the introspective results of mental activity on the basis of the objective records of such mental activity. The major portion of the important work is based on subjects of school age and persons still in the lower grades of school work. The best controlled and most complete series of investigations comes from the laboratory at Heidelberg. This group of investigations is responsible for our knowledge of the characteristics of the work curve beyond the determination of the practice curve; of the effects of rest periods of varying lengths upon the succeeding work; and also for our knowledge respecting the effect of one type of work upon another in one or two special fields of activity. As was stated earlier, the positive side of mental effort has received a much fuller treatment, but not on the basis of what can be properly termed fatiguing conditions.

Two articles, historically earliest in this work, will be mentioned somewhat in detail, the first because of its actual value as an experimental ground-breaking method, and the second as indicating exceedingly well the complexity of the problems involved in fatigue experiments. Until recently the work of Sikorski (88) had received but the barest mention and it produced no direct reactions in work by other investigators till a considerable time after its appearance. Nevertheless, he

[1] The experiments of Ebert and Meumann, Fracker, Angell and Town et al. will suggest the line of argument in this respect.

succeeded in touching on more points of primary importance than most later investigators, and certainly his work ranks well with the majority of these more elaborate investigations. On account of its general nature, we shall, however, mention Galton's paper first.

Following out a suggestion that occurred to him in one of the discussions of the Anthropological Institute, Sir Francis Galton (36) sent out a list of questions to members of the Teacher's Guild.[1] Replies were obtained from 116 teachers and these, arranged under several heads, were reported at the meeting of the Institute held March 13, 1888. He states the object of the questions as, first, to determine if possible "the signs and effects of incipient fatigue in as measurable a form as possible;" second, to discover if the teachers themselves 'had ever broken down from overwork,' and obtain knowledge of cases that had come under their observation.[2]

Among the suggestions offered by teachers as to methods for determining incipient fatigue are (1) length of time during which neatness of execution can be sustained in performing a prolonged task; (2) promptness and sureness of memory in simple things; (3) common sense arithmetical problems; (4) reaction times. The writer summarizes in two conclusions. Mental fatigue seems to differ from muscular fatigue in causing worry and nervous excitement, thus preventing recuperation; and actual overwork is more apt to occur among those who work alone (but with class incentives) and are impelled to effort through eagerness to excel. In the discussion that followed the reading of this paper, cautions were offered respecting the value to be attached to such replies and the necessity of knowing the ability of the teacher replying. A further suggestion was offered that overwork was moie imminent where the number of branches studied was limited.

[1] Rem. on Rep. by Teachers to Questions, Resp. M. F. *Jour. Anthro. Ins.*, 1888–89, pp. 157–168. The article also appeared in the *Revue Scientifique*, Jan. 26, 1889, pp. 98–103, and was reviewed in the *American Journal of Psychology*, v. 21, p. 340–1.

[2] In determining the condition of their pupils, Galton says the teachers depended on color of skin, dazed, jaded expression, nervousness, restlessness, twitchings of face, eyelids, etc. One teacher reported that she depended on

The experiments of Dr. Sikorski (88), made in 1879, were directed toward what are called psychomotor movements, meaning those movements that are voluntary, as talking, writing, etc. The theory is that fatigue (lassitude intellectuelle) will always express itself in some one of these voluntary movements, since they constitute the overt expression of ideas and thoughts.[1] Writing from dictation was selected as the best expression of the mental change. It was found that the errors made were distributed throughout a long or short dictation about equally, so no account was taken of the time in the actual tests made.[2]

the color of the tips of the ears, if they were "white, flaccid, and drooping, she concluded the girls are thoroughly weary in mind," if 'relaxed but purplish,' she concluded that they are "tired not with study but from struggling with their nerves." Headaches, fainting, sleeplessness, irritability, and despondency are all assigned as effects of overwork.

In passing to special effects of fatigue, Galton suggests a test that all teachers could try and by that means bring out its value as a fatigue test. The teacher and a number of pupils with eyes closed take hands, forming a circle. On the table the teacher places a watch, with second-hand and records the time (viz., 'chain reaction') required for a squeeze from his left hand on the right of the pupil next to him to pass once or several times around the circle. He suggests that in a circle of 12 or 15 persons this will take perhaps one second. He says, "We should expect to find uniformity in successive experiments when the pupils are fresh; irregularity and prevalent delay when they are tired."

Numerous alterations of the special senses are reported, one case of actual color-blindness, loss of memory, mechanical processes in arithmetic become bewildering, failures to grasp meaning of simple objects and sentences in reading. One instance is reported where fatigue shows itself in a 'tendency to use long words.' Galton says, 'This strikes me as a very suggestive reply,' though he fails to add just why. Out of 116 teachers who replied to the personal questions, 23 had experienced serious breakdowns, 21 of whom had never entirely recovered from the effects. They express the difficulty of distinguishing between worry and over-work and their frequent reciprocal effects. "Sustained effort, vigorous inspection, quick decision—all are impossible."

[1] "Les mouvements psychomoteurs servent à l'expression d'actes qui s'exécutent avec discernment et forment un veritable langage des idées et des pensées de même que la mimique et les gestes forment le langage des sens.

[2] The mother tongue of the children was used, but no statement is made regarding the grade of difficulty, i. e., whether this remained in any degree constant throughout the experiments. The tests were made on eighteen groups of children, the series was repeated on two classes of three divisions each, making in all twenty-four tests. In all 40,000 letters were criticised for errors.

The averages in errors per 100 letters and per 100 pupils of the classes are as follows: first class, before school work 123.56, after, 156.68; second class, 121.48 and 145.27; third, 72.44 and 102.8; fourth, 66.47 and 94.20; fifth, 61.39 and 81.06; sixth, 45.70 and 80.05, making a general increase in the number of errors for the afternoon work of about 33 per cent.

In estimating the errors only those were counted that could not have been prevented by close attention and were not due to ignorance of the principle of writing.[1] Sikorski argues that those errors due to mere ignorance and inattention vary independently of the degree of fatigue and are outside the sphere of measurement.[2]

Psychic errors or omissions and substitutions are due to a failure on the part of the subject to distinguish between *small physiological differences;* the sounds in the above letters differ, but Sikorski analyzes them as differing but little in the position of the speech organs for pronunciation. This is confirmed by the tendency on the part of the pupils when fatigued to say the words in a low tone, or to increase the size of the writing "to recall clearly the movements of the hand and thus avoid graphic errors." Psychic errors are due to defective memory, hence lack of attention; they increase after the day's work 90 per cent. He says:

> This is explained by a psychical over-excitation under the influence of which the movements become more 'jerky,' larger, and more convulsive.

An error once made tends to be repeated. Three causes appear as productive of a lower grade of work, the decrease in

[1] "On tint seulement compte de celles que l'on nomme fautes involuntaires ou inevitable,—méprises—Les fautes proprement dites sont exclues du calcul,—" "méprises" thus are "en rapport avec l'exactitude du travail du mécanisme nervo-psychique pour un temps donné."

[2] After analyzing the whole process into three parts; the auditory or visual, the inner or cerebral 'reproduction a l'esprit,' and the final transformation into writing, he assigns to each its possibility of error, omitting those of the first process from consideration. 'Phonetic errors' are connected with the central process and changes in the acoustic composition of a word as 'ainsi' for 'ainse,' 'geurre' for 'guerre,' 'tonneurre' for 'tonnerre,' etc. 'Graphic errors' belong to the third phase, the representation of the word in writing, and are indicated by changes in the usual manner of writing, as lengthening or irregular tracing, crossing wrong letters for t's, omission or duplication of parts of letters. 'Psychic errors' are omission and repetition of words and replacing a word by its synonym. Finally a group is made whose character is indeterminate because of blots and erasures. In the above order the errors distribute themselves as follows in per cent per 100 errors: 73, 11, 6.5 and 11.95; m, c, n, v, l, o, k, and d are omitted most frequently in the order given, the first five furnishing 63 per cent of all omissions. r–l. p–b, d–n, t–n, b–v, and g–k are subject to frequent substitution. (The language used is Russian.)

ability to distinguish small psycho-physiological differences, weakening of the memory, and the appearance of over-mental excitation.[1]

Without attempting at this point to determine the value of his grouping of errors, we can see that by the method used by Dr. Sikorski, the increase in the number of errors at the close of a day of ordinary school work is plainly in evidence. The general increase being almost 33 per cent. Using dictation exercises, the marking out of certain letters, filling in blanks in a page of proof, etc., Ebbinghaus (29) found in 1897 that the number of errors increased from period to period. He also found that the amount of work done increased. Laser (54), in 1894, found that the power of production increased up to the fifth hour, and the number of errors increased up to the fourth hour. But the number of those that were correct also increased up to the fifth hour, so he finds the results equivocal. Burgerstein (16) finds practically the same thing appearing. The number of errors increases up to the close of the period. Miss Holmes (42), in working over Burgerstein's results finds that his method of counting the errors is open to criticism, and concludes from her own experiments that the "complex associations of computation are affected first, those of copying, less quickly," and that the reproductive attention disappears before the perceptual.

In the more complex tests, Richter (74), giving problems in Algebra and Greek verb forms obtained results similar to those of Sikorski and Laser. Höpfner (44) dictated sentences for long periods, and found that the number of errors rose during two-hour periods. The tendency in the type of errors seemed to be to drop back into the kind of errors that were habitual

[1] Both of these articles deserved the honor of beginning a series of researches along the lines laid down. Neither, however, produced any immediate result, and they only come into their rightful importance long after the work of Mosso. Bolton says, to Mosso is due the gratitude for recognizing and showing that fatigue is a general phenomenon, but this is as plainly the conclusion to be drawn from the material gathered by Galton or the earlier work of Carrieu (18). It is quite true that Mosso, whose work we have discussed, gave us an experimental method, though even here the work of Ebbinghaus and Kraepelin's pupils has shown the value of the method suggested by Sikorski.

for that child. He does not state in this connection whether any precautions were taken to prevent a marked decline in interest on the part of the subjects used. Fredrich (35) used the same type of test and found that the work became generally poorer in the afternoon and at the close of any session of the school.

Ebbinghaus (29) used three methods: the memory method used by Bolton in memory tests on Massachusetts school children, the computation method used by Burgerstein, and what he calls the "combination method." This last was a passage of prose, mutilated by leaving out words, syllables and letters, and the omissions indicated by dashes. The children were required to fill out these blanks so that the whole would make sense. He found by the first methods that the errors increased during the day. The tests were made at the beginning and close of each hour's work. In the third method the number of errors increased as did also the amount of blank spaces filled, thus the results are generally equivocal.

Thorndike's (93) experiments on adults and on school children show results that are somewhat at variance with the results above. In tests on adults before and after a ten-hour day of mental work, multiplications were performed by three subjects. Less accuracy was coupled with an increase of speed in two cases and an increase in both accuracy and speed in the records made by one subject. The results are not checked by any observations that indicate whether the subjects were all morning or afternoon workers, or were different in this respect. Ebbinghaus also found that the accuracy decreased along with an increase in the amount of work done in the 'combination method,' but no definite relation could be found. In an experiment using additions, Thorndike found that the best work was done before the day's work. His tests on school children in multiplications, spelling, and memorizing, show only a 3.9 per cent increase in the number of errors after the day's work, though they did 99.3 per cent as much work as those tested in the morning before the day's work.

Review.—Such work as this is as a whole open to Marsh's (60) criticism regarding the possibility of a daily rhythm and

the large increase in inertia that seems to be present in the morning hours. Actual fatigue due to the work done itself is easily masked in any experiment or series of experiments by this phenomenon and by the effects of practice, as well as by the secondary phenomena of fatigue, as monotony, lack of interest, the 'glow' that comes at the close of the day's work, etc. In the second place, the whole attempt is too much in the gross to amount to more than a rough and ready method of determining the broader features of the more extreme states of mental inefficiency.

There seems to be a decided difference of opinion when the question of the amount of work done arises. In the cases cited above the general result seems to be in the more complex tests to find a gradual increase in the amount of work, and in the simpler ones to find no change or a slight decrease in the rate. The interpretation of this result has been as a rule to assign the increased rate to the effect of practice and inurement. In the second of Thorndike's experiments he finds that practice is plainly advantageous in judging the length of lines and is, in the test made, always enough to mask any effects of fatigue.

Characteristics of the work curve.—The following is in general the result of the work done in the laboratory at Heidelberg under the direction of Kraepelin (50). Beginning with the work of Oehrn (68) in 1889 and running through six volumes of the *Psychologische Arbeiten*, this group of investigators has analyzed the work curve for certain conditions of work into its essential elements. The main points that they have worked out may be briefly summarized. With the immense amount of material that must be reviewed in a few pages, it will be best to begin with Kraepelin's own summary of the work and mention in connection with it any further points from the experimental material that seem to illuminate the facts as he gives them.

The method pursued in these experiments is in its broad features the same. The addition of columns of single digits constitutes the chief fatiguing process. The length of time spent in any one series varied from thirty minutes to two hours. Two hours was the regular period for any continuous series where

the purpose was to obtain a typical work curve including a record of both the character of the changes and the absolute amount of those changes. When the series contained rest periods it was lengthened by the length of the rest periods introduced. The records were divided into periods of five-minutes each, thus giving totals of numbers added for five minute intervals through the entire time of any experiment. Additions, reading aloud, reaction time tests, memorizing the Ebbinghaus nonsense syllables, and walking, constituted the fatiguing occupations. All of these would be used where, as in Bettman's work, an attempt was made to determine the effect of one kind of mental work on another, or the effect of physical fatigue on mental work. The precautionary conditions of the experiments are, where stated, entirely satisfactory.

The factors in the curve that are found to persist and are considered typical of the curve are: effects due to practice (Uebung), fatigue (Ermüdung); the immediate effects of each of these and their more lasting effects. The permanent effects of fatigue may, if the person be normal, be disregarded. After these more important factors, he finds in the curve secondary factors, such as overcoming a preliminary inertia (Anregung); a preliminary spurt (Antrieb), of two kinds, one lasting nearly a minute, and a second which usually covers the first five-minute period; a fall that occurs before the final maximum of the period is attained, due to the appearance in consciousness of the feeling that the work is not going as well as it has been (Ermüdungsantrieb); an alteration of spurts and declines in amount of work done (Willenspannung and Störungsantrieb); and the final spurt that occurs near the end of the series or where the subject feels that he is approaching the end of the test (Schlussantrieb). This last does not appear when the end is not expected. In connection with each of these there is the individual capability of practice (Uebungsfähigkeit); the relation it bears to the power of retaining practice effects (Uebungsfestigkeit); individual endurance in the single tests (Ermüdbarkeit); or when individual endurance is combined with the initial spurt, a certain preparedness for work (Arbeitsbereitschaft); Arbeitsbereitschaft combined with Anregung results in a characteristic habituation (Gewöhnung).

Effect of pauses.—In addition to this series of general phenomena, there appear numerous changes when pauses of varying lengths are introduced into the series. These pauses may be introduced at different time distances from the beginning of the series, or differently with respect to the varying relationship of fatigue and practice in a single test. In general, this problem reduces itself to the determination of the place for the 'most favorable pause.' This pause is determined with respect to length and position in the work period by a calculation involving three principal factors—practice, fatigue, and Anregung. The formula that expresses the 'most favorable pause' when all the factors are considered is the ratio between the estimated amount of work done just preceding the pause and that which is done (estimated) just following the pause; the estimations rather than the actual figures being the criterion, because of the opposing effects of the various factors involved.

The actual place of this pause in any series of tests varies with the individual even in tests where the different subjects are working on the same sort of work. The individual characteristics that have most weight in determining the place of this most favorable pause are the capablitiy of practice, power to retain practice effects, and susceptibility to fatigue, with some indications that the grade of practice attained also acts on its location. Wimms (107) finds that 'improvability' and 'retentivity' are positively and negatively related in about an equal number of cases. The length of this most favorable pause when determined gives us knowledge of the power of recovery in the individual. Its place with reference to the beginning of the experiment indicates the control power of the subject by a balanced relation of the factors mentioned above (Gewöhnung). And its distance from the pause that is preceded by an amount of work equal to the amount which can be done immediately following, the Gleichgewichtspause (this pause precedes the günstigsten Pause), determines endurance.

We may illustrate some of these relations by the following figures, taken from the work of Amberg (1) and of Rivers (76). Amberg found that a five-minute rest following thirty minutes of work was slightly favorable. A fifteen-minute rest following

the same period of work had no effect. On the other hand, a fifteen-minute rest following sixty minutes of work was favorable in its effects. Alternating five minutes work with a five-minute rest period was unfavorable in the early stages of a two-hour test, but became a favorable pause toward the close. Rivers found that thirty minutes' rest after the first thirty minutes work resulted in a complete recovery. The same rest period following the second thirty minutes of work failed to produce a complete recovery, but sixty minutes' rest at this point was entirely satisfactory. These results are purely individual, and so far the investigations have brought out no specific place for the location of a favorable pause of a definite length. In a later article by Kraepelin (50), it is suggested that the progress of the first fifteen minutes of a piece of work as homogeneous as the addition test, may possibly be taken as a partial indication of the susceptibility to fatigue, i. e., when related to the length of the 'most favorable pause' at this point (this last to be determined experimentally). In Lindley's (57) subjects the length of the most favorable pause varied from fifteen minutes to sixty, showing the necessity in Kraepelin's judgment for the determination of this pause for each indivdual. In other words, it is essentially an indication of personal peculiarity.

Types of curve.—With respect to the work curve as a whole, Kraepelin finds five somewhat variable types. Two are known at the positive and negative practice curves, where (1) the practice effects overbalance any opposing influences to the very close of the two-hour test, or (2) fatigue is the more prominent influence from the beginning. The first curve is based on the work of Ebbinghaus and those tests where the practice attained before the test is very slight or the material is of considerable difficulty. The third 'fundamental form' of the curve is indicative of slight practice, and shows 'spurts' of speed, or practice effects, and fatigue effects within the progress of the curve, but corresponds in general to the first type. The other type indicates the balanced effects of fatigue and practice. To simplify the statement we omit here a discussion of other influencing factors. The fifth is an intermediate form based on the factors in one, three, and four.

Only one of the smaller variations in the height of the curve is considered regular enough to be related to the conscious process. That is related to 'variations in attention' and is therefore a wave of a length of about two and three-fifths seconds from crest to crest. This wave is the one described by Voss. Among the other variations given special notice, is a series that precedes the highest point in the curve; or the preliminary fall due to fatigue that precedes the final effort to keep a maximum of production. The final and beginning Antriebe are noticed. Other variations are grouped under the term 'Störungsantriebe.'

A different problem attempted by these investigators is that relating to the effect of one kind of work upon another immediately following it. Bettman found that the effects of adding and of walking were practically identical upon the intellectual work that followed. Choice reactions were slowed by the first and augmented by the second, but if the 'muscular'[1] reactions are counted and evaluated, both sorts of work actually raise the mean variation and decrease the speed of the reaction. Verbal reactions, reading aloud, adding and memorizing digits, all tend to slow the additions that follow. The heightened muscular irritability after the walk being the only distinctive difference between purely physical and mental work in their effects on the following work. The general conclusion is that physical work raises the motor excitability and produces a large increase in the errors made, while mental work tends to reduce motor irritability. The average times when the directions are actually observed, for example in a choice reaction, are reduced in both cases through the influences of fatigue. 'Premature reactions' produce the apparent difference in time of the reactions that follow the physical work.

Introspection has had practically no place in this type of experimentation, and little beyond a few cursory remarks in any of the material reviewed. One reason is of course apparent, the method of experimentation has not allowed pauses for the report of particular conscious changes, and those reports

[1] By 'muscular' reactions, Bettman (8) means those reactions that are premature, or are simultaneous with the stimulus and are thus not the result of an actual choice.

that are gathered at the end of a two-hour test, for example, are of little or no importance for the early or intervening stages of the experiment.

Wimms (107) attempted to gather some introspections from the school children he tested and reports that the method and the results seem of considerable value if the introspections be constantly checked and are made upon a large number of students. He obtains reports of a gradual increase in the 'feelings of fatigue' and in the difficulty of the task. These general statements are accompanied by reports of such specific facts, as 'muddled' conditions, a desire to sleep, weariness and muscle fatigue, all points of great interest if we are ever to correlate the conscious facts with the objective results or are ever to get any conscious facts regarding continuous mental activity. Similar reports are mentioned in Sikorski's paper. The muscle fatigue was in this case speech-motor. Galton's questionnaire shows the wide range of material possible in this connection. Joteyko (47) believes that intellectual fatigue arises suddenly in contradistinction to the slow rise of muscular fatigue; a belief that doesn't seem to be supported by the major portion of the work that has any bearing on the point. To cite a single instance, it seems from the experiments that there is no certainty that voluntary muscle fatigue, or what passes for muscle fatigue in the normal organism, is any more gradual in its rise than is the appearance of a 'sensation.'

General conclusions.—This incomplete and very imperfect survey of the work of Kraepelin and his pupils indicates the great importance that must attach to the objective record in the estimation of qualitative and quantitative changes in mental work. The distinct failure of the objective record as it has so far been applied seems to be its inability to cope with the situation more in detail. It fails to account for the variations as they seem to appear in the most cursory observation of different persons in their habitual methods of working. Comparability of conditions is absolutely necessary for the comparison of records from different individuals. It is also necessary to determine whether the imposition of, for example, the highest rate of speed, a certain kind of subject-matter requiring

a single type of imagery, or a specific mode of reacting, or even of recording, does not alter the conditions more materially than they are otherwise equalized by the setting of certain objectively similar conditions. One person may be affected by the set of conditions imposed so that his imagery is of a type not ordinarily used, another finds that the demand for a high rate of speed is inimical to his usual practice curve, etc. In short, an investigation of the individuals of a group must be made on the basis of their habitual methods of working before work curves can be made of definite value in any series of generalizations.

The question of habituation is one that relates itself to fatigue in an intimate way, though we can merely touch its problems at this point. From the side of physiology, the investigator has begun to present us with all manner of rhythms by which organic processes are regulated, and among the latest of these are the larger muscular rhythms. The facts regarding the number of nerve impulses that a muscle will respond to separately within a given time have been known for a number of years. The relation of the load to the working power of a muscle has also been a matter of common investigation. But the majority of the investigators on fatigue in both fields, muscular and mental, have worked with the express purpose of obtaining fatigue in the shortest time possible. Thus any larger rhythms that might have been present under normal conditions have been obscured by this specific aim of fatigue experimentation. It is a matter of common observation that the workman has his individual speed. The work of Marsh (60) lately has shown distinct daily rhythms. The possibility of shorter rhythms lying between these larger periods and the short attention waves has only lately begun to receive special notice. For example, the work of Hall has shown that there is a definite load and rate for the muscle that will enable any single set of muscles to perform their work for practically indefinite periods of time; so that under these conditions, which he speaks of as normal, muscle fatigue of the exhaustion type is eliminated. Habituation is, in fact, the establishment of just such 'physiological' rhythms, and work done under condi-

tions that do not approximate these habitual rhythms or attitudes is abnormal and does not present to us a clear picture of the properly working organism.

Psychological rhythms follow with considerable accuracy the general picture we have drawn for the unconscious ones. Fatigue, exhaustion, weariness, total distraction, break up or destroy such physiological rhythms. Considerable work has been done to show the effect of varying amounts of distraction upon any piece of work in its various features. Distraction of a very limited amount tends to aid in the production of work; music aids in the maintenance of a rhythm; greater distractions, or those that are incongruous, are fatal to the rhythm (17). The introduction of these slight distractions tends to remove the 'feelings of fatigue' where they are of an inconsequential nature, and in cases of high mental excitement, not merely in inurement as Bolton (12) maintains, these warning signals may be disregarded altogether, disregarded in the sense that they are not present consciously till exhaustion occurs.

In connection with this problem of activity, we have so far failed to mention, except in passing, the 'feelings of effort' and the consciousness of strain that accompany any sort of work that is slightly different from the normal or varies from the purely habitual. These feelings are the accompaniments of both muscular and mental expenditure of energy and are usually conceded to be peripheral in their origin, arising in normal conditions almost entirely from stimulation within the proprioceptive group of receptors. Waller (101) makes the 'sensations of fatigue' simply extreme forms of these sensations.

Mosso showed with the ponometer, a modified ergograph, that the amount of effort expended in raising a weight increased rapidly during a muscle fatigue test. Trêves (95) working on the same problem found the same general result, but finds in addition that the expenditure of effort is in no degree so regular as is the inverse change in muscle work. Effort is expended in waves of force that approximate what he calls the irregular progress of mental fatigue rather than the regular decline of muscle power. He finds that a muscle under strain, such as continuously supporting a weight, withstands the strain steadily

until exhausted. This applies to an extreme strain rather than to the moderate forms of muscle tension. These results correspond fairly well with what has been done, principally by way of observation, in the field of mental effort. Exhaustion is always a sudden phenomenon and the more moderate rates of energy expenditure are, so far as concerns the objective records, irregular in their character.

III. EXPERIMENTAL. PROBLEMS AND INVESTIGATION.

A. Some Specific Problems.

As one works over the material in each of the particular fields, the special need seems to be for more and more experimental work. The moment we get beneath the surface complexity, there appears a multitude of individual problems, each offering, apparently, its quota of material for the solution of the more general problems. The need is now, however, not only for more work but for a specialization in the various fields. The data, as will appear from the above sketch, that must be presented within a review on 'fatigue' are entirely too complex at present to admit of unification. So many divergent paths are presented that the thorough examination of any one has so far failed to attract any considerable number of investigators, and until a division of labor occurs there seems little chance of the consummation sought by Seashore in his plea for a better technique in this line of work.

Intellectual and physical work.—In differentiating between intellectual work and physical work there are said to appear certain very distinct characteristics. In physical work, the characteristic usually assigned is the gradual decrease in the curve of work. This gradual decline is assumed to indicate the way in which a muscle becomes exhausted. When the energy of contraction is taken into account, the curve of work becomes more irregular and shows variations that are assumed to be indications of the central changes. The muscle therefore is supposed to fatigue at a steady rate and the motor responses of intellectual work are also expected to fatigue in this straight line curve.

When we come to intellectual work there is reported a distinct difference. Joteyko says that intellectual work goes on without any apparent diminution in ease, or in loss of practice, or in increase in errors. She finds however, that 'sensations of fatigue' appear suddenly. The sudden appearance of these sensations is what she calls intellectual fatigue.[1] Trêves indicates suddenness as the prime difference between nervous fatigue and muscular fatigue. The principal difference then that appears between these two types of work is one of slow decline in the case of physical fatigue and of sudden appearance of certain distinct sensations in the case of nervous fatigue, fluctuations in the 'objective manifestations of work.'

Kraepelin finds that the loss of capability, while it appears at a certain stage in the work, is nevertheless due not to a sudden onslaught, but rather to the gradual decline in capability to retain practice effects and to maintain the work at the same high rate attained at earlier points in the two hour work curve. He finds that practice increases in a decreasing ratio and that fatigue increases in an increasing ratio. The result of these opposing influences is that the curve at a certain point changes from a rising curve to a falling curve. This fact does not indicate the sudden appearance of fatigue, but indicates the point where fatigue overcomes the practice effect. Sensations that are to the subject 'fatigue sensations' are simply signs of weariness and due to the monotony of the work. They can be pushed aside and the work continued for some time at higher speed. Intellectual or central fatigue comes when the curve of work begins to fall and cannot be brought back by any effort of the subject.

The difference in these two views seems to be plain. The difficulty is, in the first instance, one of statement merely.

[1] A question arises here; does Joteyko mean in the case of intellectual fatigue, merely 'sensations of fatigue,' or the actual loss of capability to work? If she means sensations alone, then certainly she is right; for all sensations, as far as the psychologist can tell us at the present time, arise suddenly; they are due to the shock change which occurs in consequence of the presentation of stimuli. But if she means a loss of capability to do mental work in terms of nervous energy fluctuations, then we would question the accuracy of her observation, in so far as it applies to the objective manifestations of work, at least.

Kraepelin is basing his conclusion on a single series of addition experiments and on the definition of fatigue which says that fatigue is a loss in the ability to do intellectual work. Joteyko's definition of fatigue refers seemingly to the sensations alone, and not to the ability to do work. If we try to combine these two definitions, we still find, however, that there is a distinct difference of opinion.

The work curve as Kraepelin investigates it, is found by a combination of the methods employed (*a*) in a purely intellectual effort without recording and (*b*) in making the motor record of intellectual effort or of physical effort. There ought then to be, as far as results are concerned, a distinct difference in the type of curve. The intellectual work curve ought to show more variability. It ought to show the entrance of disturbing factors such as change in attention, and the rise of different ideas; and it ought to show the effects of sensory or motor fatigue.

The further question arises. Does sensory fatigue, so far as isolable, show any different characteristics from mere motor fatigue? Few curves of purely sensory fatigue have been worked out as yet. Where sensory fatigue has been determined, however, the curve is more or less a straight line curve. This appears in such curves as are obtained from the olfactory senses, from taste, and similar sense fields. In the reaction time experiment we have a case where both sensory fatigue and motor fatigue are possible. Scripture shows that sensory fatigue is a prominent factor. Eliminating the motor elements one by one, he shows, as has been mentioned above, that the curve of fatigue as shown by the increase in length of reaction time and by increased variability of the time of reaction, or mean variation, is decreased very noticeably. This curve is practically equivalent to the muscle curve.

Objectively we can undoubtedly arrange mental calculations and associations so that they will give, when recorded, practice and fatigue effects that make a straight line curve as Kraepelin has done, i. e., taking them separately. But there might go along with this the greatest possible number of fluctuations from the side of introspection. Or the reverse is possible, to

set such a task that introspectively the subject *feels* that he is maintaining a steady standard in rate and quality of work and with this obtain very considerable changes in quality and quantity of work objectively considered. To repeat, sensations of any kind are more or less sudden in their nature; they do not come gradually into the field of attention. If these alone be fatigue, then our problem is already solved. If, however, we are dealing with a real diminution of work, our problem remains. *Does the appearance of 'sensations of fatigue' bear any relation to variations in the objective work curve?*

Relation of the 'muscle fatigue' curve to the 'intellectual fatigue' curve.—The conditions so far discussed are conditions which are maintained in both intellectual and physical work. The intellectual work is not itself recorded as actually done, however, but only the motor reactions which are made in objectifying the records. The result of this situation has been entirely to obscure the greater portion of what may be genuine intellectual fatigue phenomena. Lombard, Trêves, and others have shown the complexity that lay hidden in the 'straight line' muscle curves of voluntary contractions. In some few instances, such as the reaction time experiment, we can see that intellectual fatigue shows itself not merely in sensations of fatigue, but also in increase in mean variation, in an increase in errors made, and a possible decrease in the amount of work done. This quantitative decrease does not show itself as definitely as do the other phenomena. The reason for this seems plain. In cases where a motor record of the experiment is made, the motor portion of the whole sensory-motor arc is the part which controls. The intellectual 'give and take' of the situation is obscured by this necessary and fundamental response factor.

Let us see if a situation similar to this does not also obtain in much of the work so far done in muscular fatigue. In Lombard's investigations the curve starts and works down in a 'straight line' curve to the point of no contraction; then starts over a second time and so on, making almost any number of these 'straight line' fatigue curves. This rise and fall occurs in a single experiment. Mosso claimed in his early experiments that to get the same fatigue curve over it was

necessary to wait two hours or more after the first experiment. Naturally he did not mean that the muscle could not work during this period. But he did mean that the curve was materially changed until after two hours' complete rest. Lombard found that the curve could be run for twelve or fifteen minutes and still obtain contractions one-half or two-thirds as large as in the beginning. The phenomenon of the 'Treppe' still further modifies our idea of the 'straight line' muscle curve when the muscle is working under approximately normal conditions.

This situation seems to indicate that we must materially modify our idea of the fatigue curve in muscular work. W. S. Hall (39), in a series of experiments mentioned above, finds that if the conditions of ergographic experimentation are modified slightly, the exhaustion 'fatigue' curve does not appear at all. What the situation would have been had Mosso used such a type of experimentation in testing the effects of mental fatigue on muscular work we do not know. We do know in Lombard's experiments that voluntary effort ceases for only one or two contractions, and it begins again with a vigor which is almost equal to the primary effort.

In Kraepelin's experiments we find that the method of recording is distinctly different. Kraepelin himself makes no remarks with regard to his method. We learn, however, from the descriptions of his experiments that what actually takes place is a very rapid movement of the fingers and of the hand in recording the additions made by the subjects. In the tapping experiments carried on by Dresslar and others, we find that the fatigue curve is quite similar to the actual 'muscle fatigue' curve as made by the ergograph under modified conditions. Is not the fatigue curve that Kraepelin obtains, a curve that is made up in great part of purely 'muscle fatigue' phenomena? Amberg thinks not, at least in those subjects where the recording is relatively faster than the calculations, but he only finds one such case. However, the amount of work necessary to record one figure every time one is added is not inconsiderable. And in addition to this it takes up no inconsiderable part of the time of the entire experiment. The work recorded

below in the experimental portion of this paper shows quite plainly that no simple motor response can act with a speed equal to the details of mental imagery. This last factor is not definitely met in any of the discussions of these experiments.

The other factor is, however, the one we wish to emphasize here. Where only one or two fingers are used and a small portion of the arm, does this motor reaction not take on practically the exact conditions of the muscle exhaustion 'fatigue' experiments? And if so, may we not expect to find a gradual decrease in the actual amount of work done due, not to any 'intellectual fatigue' induced, but rather to an actual muscle fatigue? If so, we may expect to find that the curve is a regular one and takes on the characteristics of the simple *muscle exhaustion curve.*

The only modification of importance that would appear in this curve is one due to the large practice effects that always appear in this kind of work. In the ordinary muscle curve, inurement effects do not appear except when the experiment is carried over a long period of time. That they do appear, however, when experiments are made long enough, is shown by the work of Bolton. The practice effect in the case of additions is in the first experiment a very large one. None of the subjects in Kraepelin's tests carry the work far enough to reach what is known as the automatic stage in the additions. The results show that practice effects were prominent in each day's experiments. The Uebungsspur also appears in the effects that remain from one day to the next up to the very close of the series of experiments.

The difficulty in evaluating Kraepelin's experiment rests, in addition to this fact, upon the lack of data regarding the number of errors made by the subjects used. The *quality* of the work was not taken into account in any of the tests. Exceptions to this extreme statement may be noted in the work of Amberg and Voss, where both make specific mention of errors, Amberg to the effect that in his work the errors are only a slight per cent. of the total additions, giving as the highest per cent 0.0917 per cent, and for corrections, 5.38 per cent of the whole. Says Voss in this respect:

Wir glauben das thun zu durfen (omit errors) da einerseits die Deutung der gefundenen Fehler, nicht in das Gebiet unserer Arbeit gehört und da anderseits wie Amberg best gestellt hat, die Fehlerzahl schon an und für sich gering ist und mit der Uebung noch stark abwinnet.

I do not find that Amberg is at all certain he has shown this last point, for he finds the errors too few and scattering for any definite conclusions.

Apparently, the motor reactions of any subject, if made complex enough and not held to an exhaustion series of reactions, will give a curve similar to the fatigue curve that Kraepelin finds. Practice effects may continue up to the close of the experiment, and no fatigue effects appear in the curve. For example, when eye muscles are eliminated one by one, variations due to fatigue decrease in the reaction test. The practice effects, however, remain, and the reactions become shorter and shorter up to the time when the experiment closes. *The first general problem before us then is to determine what are the true conditions of a purely intellectual fatigue test, if such there be.*[1] Naturally these conditions cannot be determined except through a long series of experiments. So in the work here reported the conditions selected were such that we might decide to some extent at the close of the experiment whether we had obtained any new set of conditions by the change in the results obtained. The problem as thus stated is not one that we hoped to solve outright at this point, but rather to attack with the view of obtaining a method. In this respect, the paper follows in a greater or less degree the suggestions made by Hall and by Mrs. C. R. Squire (90) in their work on fatigue tests, as well as the work of Kraepelin, Sikorski, *et al.*, who have attempted to separate the conditions necessary for practice improvement from those appearing in fatigue and inability to retain practice effects.

Quality of work done.—In any case there is a necessity for making a record of the intellectual work done, if either the quantity or quality of that work is to be measured. If we are

[1] Bonser's (13) investigations indicate the value of such researches though the actual results so far obtained are necessarily small.

simply seeking for quantity, it is possible to get a fatigue curve by the ordinary methods of recording and of carrying out the experiment. By this we mean that the motor reaction may be used at the highest speed possible. *When used at this maximum speed, our contention is that as far as quantity is concerned the predominating factor is a curve of simple muscle work only slightly modified by the sort of intellectual work recorded, and the muscle curve so obtained is not the significant one as far as the intellectual work is concerned.* For a fatigue curve which is an indication of the quantity and quality of the intellectual work itself, we must reduce this purely motor response effort to a minimum. Further, this reduction of the motor element cannot be achieved, if the record be made by any of the more mechanical means, or if it be made at the highest speed of which the subject is capable. It may be possible to use the usual methods of recording if the speed at which the work is done is made what may be called, following Hall's phraseology, a 'physiological' speed. The only method we know of getting this habitual physiological rate in the case of making a record of intellectual work is to allow the subject to choose his own speed.

Fatigue and practice.—A second problem deals with the relation supposed to exist between fatigue and practice. Kraepelin's conclusion seems to be that these two factors are definitely correlated. Our first question may be stated thus: *Is fatigue actually correlated with practice in the way in which such experiments seem to indicate?* This question applies to fatigue of the type known as loss in power to produce work, a decrease in capability. The second question relates to the matter of errors as they appear in work where errors can be evaluated, and may be stated as follows: *Do errors as they appear in the work, come in an increasing number per unit of time, or do they decrease as practice effects begin to appear;* in other words, *are the errors an indication of the results of practice, rather than an indication of the stage of fatigue which the subject had reached in any one experiment?*

It may be well to state in this connection that certain other results obtained and mentioned above, such as Thorndike's

and Wimm's, do not wholly agree with the results indicated by Kraepelin. The three hour test used by Thorndike shows no fatigue effects at all. In the experiments on school children, he finds that the errors increase as the work goes on, but he also finds that the amount of work done per minute increases. Going back to the work of Ebbinghaus and those experimenters who have attempted to use purely intellectual tests with no, or very little, motor recording, we find that in the majority of cases the number of errors increases as the experiment is continued. In about an equal number of cases the experimenters find that the amount of work done at the end of the period of the experiment is equal to, or greater than, the amount done in the beginning of the experiment. The other results seem to show that the amount of work decreases as well as the mean variation in number of errors.

Evaluation of errors.—Another problem closely allied to these relates to the appearance of errors in any single memory experiment. Do they constitute any distinct relation to the practice curve or to fatigue as a loss in capability to do work, or are they independent of both? In either case, it certainly seems an important problem to determine what the facts are and whether any test for them be obtainable. As a secondary problem, we may inquire whether the errors as they arise change in their character from the beginning of the experiment to its close. Do they increase in number; are they more prominent and numerous at the beginning of the experiments or at the close; is their distribution due to the effects of practice; or are they scattered throughout the series without any reference to the work curve as far as amount of work done is concerned? The major portion of the errors will doubtless depend for their quality and number upon both the actual difficulty of the intellectual work itself and certain changes that occur in the effects of practice and fatigue throughout the series. The correlation of quality of work with practice and fatigue will necessitate an attempt, at least, to classify the kind of errors that occur.

To summarize: We have sought to obtain a method of recording that will eliminate almost entirely fatigue of the

muscles involved. This is further to be provided against by using habitual rates of correlation in thinking and recording. The mental material or work is simple, but difficult enough to give distinct qualitative results. As a result of these conditions, we hope to get definite answers to certain questions. Is there a difference between mental work at laboratory speed and that according to established habits? What further can be said about the actual nature of the work done, i. e., with respect to quantity and quality? Is there any value in persistently demanding introspective accounts with such experiments?

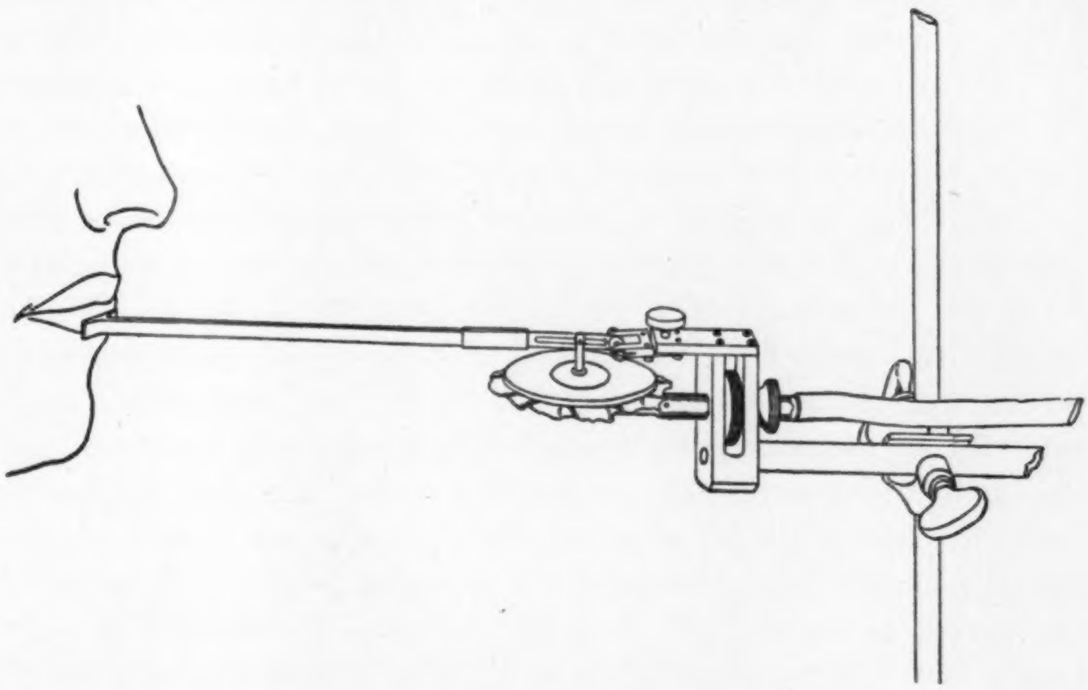
B. Conditions Selected for Experimental Tests.

The specific arrangement of the experiment is quite similar to that used by Mrs. Squire in her tests on fatigue in this laboratory. We used practically the same mode of procedure with certain changes and modifications noted below. The test is primarily a memory test.

A series of ten digits was memorized and tapped; a typical one being as follows: 4, 4, 2, 1, 3, 2, 2, 4, 3, 1. By 'tapping a digit,' or a series, is meant the movements of lip or finger made in groups or series of groups and finally recorded by means of lever and tambours on the smoked paper. In the series above, for example, four movements of lip or finger were made, and after a slight pause four more movements followed. Again a slight pause and the two movements corresponding to the third digit of the series were tapped. This procedure gave on the drum record a group of breaks in the pointer line that corresponded in number and order to the number and order in the series memorized before the experiment began. The movements of the lips themselves in the lip tapping correspond almost exactly to the *first part* of the movement involved in closing the lips for the letter 'b.'

Apparatus and Method.—There is little need of a detailed description of the apparatus used in making the record. The major portion of it is that which is wholly familiar in every laboratory. The Marey tambour with a wooden lever about five inches long, transmitted lip or finger movements to the smoked paper. The connection with the lip, lower lip usually, was made by a narrow

strip of paper or metal so attached to the end of the lever that it could be changed for each subject. This strip was about one centimeter in length and one-third as broad, and was fastened at right angles to the tip of the lever of the receiving tambour. It was kept in position on the lip of the subject by the amount of pressure needed to make the subject conscious of this movement in connection with the number of taps to be made, four, six, etc. After the preliminary trials the subjects report no feeling of discomfort or of fear lest this should become disarranged, and it could even be shifted during a series without confusing the record.



The receiving tambour was held in position by an ordinary standard and universal clamp and could be arranged by the subject to suit his own particular habits of sitting while reading or writing. It could also be shifted during the experiment if the subject so desired. This last possibility was needed but a few times, as the subjects found the position could be arranged quite satisfactorily in the beginning of the experiment.

From this receiving tambour, rubber and glass tubing enabled us to transfer the record to any distance desired. After some preliminary experimentation in which different tables were used and even different rooms, it was found that the apparatus could be made to run regularly enough to allow all of it to be placed on a single long table. This final disposition of the apparatus was an arrangement that put the recording drum but a few feet away from the subject, either on the same table or on another standing near. Sight of the drum or the record was prevented by a large paper screen placed about two feet from the subject. The table on which the apparatus was placed was a large heavy one of oak,

and very solid and steady under any jars of the apparatus or of the motor. Practically nothing of the vibration of the motor was transmitted to the subject.

The drum used in recording was the Scripture horizontal drum with wheel attachment for leather belting. To drive this drum at as uniform a rate as possible, a small motor run by electric drop-light attachment was used. The Pillsbury speed reducer enabled us to control the rate of the drum and conserve the use of smoked paper. This was further cared for and the records made more uniform, by running an endless piece of the smoked paper over the Scripture drum and a second drum at a distance of about seven feet from the first on the same table. This made a sheet of about fifteen feet of smoked paper, and enabled one to sit down at the apparatus and run off a record thirty minutes in length without any change in adjustments. The paper was smoked in the ordinary manner after being placed on the drums. After the record was made it was floated with the shellac and alcohol preparation and allowed to dry before being taken down. With paper of the right thickness this can be easily done by applying the preparation to the reverse side of the record, as is done in charcoal work.

Along with the record of the lip or finger movement, a time record was made. This was done by putting into single circuit a Jaquet time-marker and a pendulum clock with electric attachments. The first marked seconds, while the second was set to mark the half minutes. They were adjusted to run together so that the record as it appeared on the paper gave the usual short break each second, and at the half minute gave one somewhat longer. This method of recording enabled us to detect variations in the record within the time of a second. This time record obviated the necessity of taking account of any possible irregularities in the rate of the motor.

The clock was kept in another part of the room, and all the apparatus on the table was muffled by cotton batting and cork supports. This was not entirely satisfactory since in one or two instances the subject reported at the end of the test that he consciously selected the rate of the throbbing of the motor for his own tapping rate. This point was examined in the records of other subjects, and kept in mind in reading the rate of tapping. No selection of the rhythm of the motor could be detected either in a regularity among the records of the subjects, or in any sudden and continuous change in speed in any single record. In the majority of the records the irregularities are of such a nature as to permit of no such possibility. Even if such an objective rate were selected, the results obtained would only be made more definite and certain in their intrinsic character. Those records run with the motor in another room showed no differences in this respect. In but one or two of the series did a subject report consciousness of the movement in the apparatus or of the noise in the room. This last was reduced entirely to that arising from the

apparatus, since the experiments were all carried out in a large basement room free from noise beyond that incidental to any room that is not entirely sound proof.

Material of Tests.—The experiments may be divided into four different groups. The *first* (I) was a practice series intended to acquaint the subject with the apparatus, to test the effects of practice on work carried on at the will of the subject as far as speed was concerned, and to test the effects of practice on the changes in number and kind of errors. This series in other words acted as a control test.

The *second* group (II) consisted of seven experiments, of somewhat irregular character, arranged to determine the effects of practice and the increased difficulty of any problem set, on the results of the work itself and on the kind of fatigue produced. 'A' of this group was the series of ten digits used in the preliminary practice series and constituted a second control in a portion of the subjects used: in the others it merely marked the first of the tests after a short series had been used as a practice series. These will all be indicated when the results are tabulated. The other six of the group, 'B,' 'C,' etc., with the exception of the sixth series of the group, are much more difficult than this first one. They consist of a series of consecutive digits, ten in number, that are to be memorized before the tapping is begun, and *also* two or three calculations that are to be performed on the series during the experiment. The kind of calculation to be used was communicated before the test began, and memorized, so that no directions were needed by the subject during the course of a single experiment. This last condition is entirely true of the 'B' and 'C' of the group only. In the remainder of the seven the conditions are somewhat different. *Two sets* of ten digits each and the calculations that went with one of them were memorized by the subjects. No trials were made where both series involved calculations. The subject was started on one of these series and allowed to continue till the word 'change' was spoken by the person in charge of the experiment. At this point, the subject was expected to begin the second series which had been held in memory till then.

In detail the series used were as follows: as practice series and the first or 'A' of the regular tests, the series 4, 4, 2, 1, 3, 2, 2, 4, 3, 1, was used; it also served as the first part that preceded the word 'change' in series 'C' and for that part of the series that followed the direction to change in series 'E' and 'D.' For series 'B,' the following served:

3, 2, 2, 4, 3, 2, 4, 4, 2, 3,
subtract one,
multiply by two and subtract one,
multiply by three and subtract three.

Series 'B–2' was as follows:

1, 1, 4, 2, 1, 3, 2, 2, 1, 3,
add one,
multiply by two and add one,
multiply by two and subtract one.

Series 'C' began as described above, and at the signal changed to the following:

2, 1, 3, 4, 1, 1, 2, 4, 2, 3,
multiply by three and subtract one,
add three.

Series 'D' ended as described above, and began with the following:

3, 1, 2, 1, 4, 3, 4, 1, 2, 1,
multiply by three,
multiply by three and add two,
multiply by two and add three.

Series 'E' was much less complex, consisting of twice the original series and at signal the original. While series 'F' was the last of this group and probably as far as complexity was concerned the severest test of the group; the first part of the test was as follows:

3, 1, 2, 1, 4, 3, 4, 1, 2, 1,
multiply by three,
multiply by two and add three,
multiply by three and subtract two.

This was continued till the signal to change was given, when the following series was taken up and tapped, having been committed to memory before beginning the tapping of the first part of the test: 2, 4, 2, 1, 2, 4, 1, 3, 3, 2, with no calculations.

In this group, group II of the tests, 'A' was intended to serve more or less as a control, as it was used by some of the subjects for the first time, and by the others after the practice group. 'B' and 'B–2' constituted two of the same character and if possible were to be used in evaluating the effects of practice in any single test. In the others an effort was made to eliminate practice and to test the effects of a transition from a difficult piece of work to one that was more or less habitual (in some of the subjects practically automatic), or the reverse transition.

Eight subjects were used on this second group of tests, five of whom had practiced on the first group mentioned above. They range in types of imagery

from the highly visual to the highly motor; the range being practically the extremes of these two types. One of the subjects is so definitely motor in his type that he is unable to find any other sort of imagery in his thinking. None of the subjects used were highly auditory, though in some cases this kind of control was used in remembering the series. All of the subjects were graduate students and instructors in the department. All were therefore trained introspectionists and capable of carrying on an experiment from the standpoint of the interest of the work, rather than needing an intrinsic interest in the character of the work itself.

The third series (III) of experiments here reported was of the same type as the above, with this exception in the conditions of the test itself. The subject was directed to memorize as above and carry out the calculations indicated, but instead of selecting the rate that seemed most favorable to the continuance of the experiment, he was asked to carry on the tapping as rapidly as possible, compatibly with avoidance of tetanus of the lip or finger. This is the usual direction in tests of this kind and was used here as a control over any possible irregularities (or regularities) that might arise through imperfections in the apparatus and thus cause an artificial record; secondly, it was used as a control test for the former series.

A final series (IV) was attempted in which the subject did the same sort of work as in group two, under all the conditions there described, with the exception of the manner of making the record. Instead of using simply the lip or one finger in making the record, the subject was asked to tap the series with both the hands and the lips in turns. The forefingers of each hand were used and the lower lip as above. These records were not made simultaneously but alternately, and went into the same line of the tapping curve, thus giving, not three records, but one as before. It was possible to determine which movement had made any portion of the record by arranging different degrees of tension in the tambours and by an arrangement of the experiment which asked for the alternation of hand, lip, hand in a definite order. Only three subjects carried out this test in detail.

The time asked of the subjects in these experiments was rather exacting in all the groups, and in group II especially where the subjects were asked to come to the experiment immediately after at least one hour of exactly the same sort of work (the laboratory, library work, etc.) and at the same hour of the day for the seven consecutive days used in the series. The remainder of the tests were not carried out so rigorously, but the same time of day was insisted upon for the completion of any particular series or group. All of the subjects did not come at the same hour of the day but hours were selected, generally in the forenoon, when their work was regular enough to admit of the above directions being carried out, or in the afternoon, if the subject regarded himself an after-

noon worker. No outside artificial work was imposed upon the subjects at any time, though the subjects were asked as far as possible to make the days on which the groups came as nearly typical as possible, and where this was not carried out through unavoidable circumstances, special note was made of the condition in which the observer came to the experiment. No specific diet was suggested, but those things that the subject considered especially stimulating or 'fatiguing' in his own case were omitted from the bill of fare during the continuance of the group of experiments, or when not so omitted noted in the preliminary notes.

Of the persons who gave of their time and energy to this work, A., Sd., Rhn., and Ym., were men. Ch., Dy., Rn., Fl., and Wl., were women.

This type of test handled as it was is primarily a memory test. The introduction of the calculations into a number of the later series modified this slightly but mainly prevented complete automatism. The subject was expected to memorize the series of digits before beginning the tapping. When the calculations were introduced he memorized, in addition, the *processes* that were to be carried out during the experiment. He did not at any time *perform* the calculations before beginning the tapping of the first series of digits.

Regarding the length of a single test there is some question. Undoubtedly a few of the tests ought, for purposes of control, to be run to the length at which we should expect to find distinct results of mechanical fatigue or of exhaustion. None of the problems as we have stated them above actually demand such results. It was decided, therefore, in carrying out the majority of the experiments not to run them beyond the time selected by Kraepelin as sufficient to indicate the general nature of fatigue in the individual subject. This time as he determines it is between fifteen and eighteen minutes after the beginning of the experiment. To be sure, in so arranging the experiment we do not get the actual work curve. What we do get, however, is a chance to determine the *tendency* in all of the characteristics mentioned above.

The length of the experiments was varied for the subject each time. There was thus no chance of a final spurt or attempt to eliminate errors as the subject felt the end of the test approaching. The actual time spent in making a single record varied from fifteen minutes to thirty minutes. The subject at no time knew exactly when the experiment would be terminated.

An attempt was also made to eliminate mechanical practice by giving a series of practice tests on a simple series of digits for eight to ten days preceding the actual experiments which were to be used. This practice series will be referred to in the report made on the tests. An attempt was made, however, to keep the interest of the subject aroused throughout. All of the subjects, notwithstanding, report wandering attention and actual loss of conscious control over the subject-matter of the experiment. In one or two cases the subjects became

entirely confused and were compelled to start again. In some of these cases the cause is easily traceable to the emotional excitement of a new type of recording; for in using the same series of digits with the same subjects and allowing them to tap with the fingers, instead of the lips, they carried the experiment through with no more effort than was reported by other observers. Practice eliminated any conscious lip fatigue in all cases.

Isolated exceptions were plainly due to the arrangement of the apparatus or to the immediate condition of the observer during the experiment. These instances are reported in the introspections that accompany the records.

Collocation of Results.—In determining the factors represented in the results, a large number of tables and curves were made, all of which seemed necessary to determine the nature of the work done and the origin of the changes that occurred. These tables and curves are far too numerous to try the patience of any possible reader with all of them, so from the number we have attempted to select some to show the different methods used in collating the results; and to select others that seemed typical of the whole, or that indicated the relation these experiments bear to the work of other investigators. Finally, certain tables show what seems novel in the use of this method as applied to intellectual work. It is unavoidable in selecting only a few of the large total of tables and curves that some illustrations will be omitted which certain readers might find helpful.

An effort has been made to present these results in the following order. (*a*) The various facts that appear in connection with changes in speed and amount of work done have been discussed first, since these are the most prominent part of the results obtained by other psychologists and therefore must show at once the relation these experiments bear to earlier work. (*b*) In the second place, the facts have been assembled that have come out regarding the "grouped" appearance of errors and their quantitative distribution in the continuous series of work; and thirdly, (*c*) an attempt has been made to determine the character of these errors in relation to their distribution. (*d*) In the fourth place, a partial correlation of the subjective experience of each subject with the more prominent changes in the objective records of the subject is offered in the hope that it will stimulate other introspective accounts. In the conclusions, we have attempted to bring together the general and specific results with reference to some more definite explanation of the phenomena than has as yet been offered.

C. *Detailed Discussion of Experiments.*

a. Practice Tests.

The preliminary trials were carried out in detail on four subjects, five including the writer. These habituation tests were made with the series of ten digits, 4, 4, 2, 1, 3, 2, 2, 4, 3, 1, and were carried out under the conditions set for the experiment and described above. In all of the subjects the tests were continued until the series had been thoroughly learned and any errors that occurred and were noticed, were reported by the subject as due solely to mind wandering. The automaticity of the process finally became so great that it enabled the subject to turn the attention from the immediate process for periods that covered the time spent in tapping two and even more digits. The results of these tests are interesting from the standpoint of practice, changes in rate from one experiment to another, and the variations in errors that appear.

Nature of errors.—The errors that appear in this test are of four kinds; one, where the digit appearing in the record is larger than the proper digit by just one tap; those where the digit or digits are repeated, as 3, 2, 2, 2, for 3, 2, 2, and 4, 3, 4, 3, for 4, 3; a third sort is the omission of one or more digits, as 3, 1, for 4, 3, 1; the fourth, of which a very few occur in these tests, is where one or two taps are made and these followed after a short interval by others, evidently intended to finish the tapping for some digit larger than either of the groups. This occurs for the larger digits somewhat infrequently in these series. Once or twice, but not oftener, less than the requisite number of taps has been made in this test, showing possibly some uncertainty on the part of the operator regarding the proper digit to be tapped. The majority of the other errors come from distinctly different sources. For example, omissions and repetitions are both very uncertainly reported by introspection. The omissions are particularly difficult to catch in this way, while for the writer the over-tapping was practically always detected in these simpler series. Finishing a digit slowly is usually a plain case of conscious effort to orient one's self in the series and in the tapping of the digit.

Little can be said with regard to the rate of tapping at this point in the practice. As the control over the mechanical difficulties of the apparatus and over the ten-digit series increases in accuracy and ease, the rate of tapping gradually increases. All but one of the subjects show that they have taken advantage of the direction to take a rate that is fairly safe and as habitual as the novel character of the experiment permits; i. e., they begin the tapping at a much slower rate than the difficulty of the test demands. One subject, 'R.,' began so near this safe limit in rate that in consequence she does not show the same changes that are exhibited by the others. This subject is distinctly of the motor type in her imagery. No method of questioning that would elicit an appropriate answer and still leave the subjects in the dark with regard to the nature of the experiment succeeded in bringing out any consciousness on their part that there had been any change in rate during an experiment. None were at all sure that they had changed the rate from one day to the next, though all reported greater ease after the first few minutes of a test and after the first days of the group. The 'at will' rate which is a part of the directions for this test shows itself in the loss of the 'Antrieb' and in a relatively much larger 'Anregung.' The other factors of the Kraepelin work curve seem constant.

b. Control Tests in Practice Series.

After this preliminary practice on the simple series of ten digits, 4, 4, 2, 1, 3, 2, 2, 4, 3, 1, the series was complicated by the introduction of a second set of directions. In this particular test the subject was asked to add one to the original pattern, to multiply it by two, multiply by two and subtract one, and finally to multiply by two and add one. When this had been done the whole set of directions was to be reversed twice without repeating the simple series, and at the last reversal of the directions the original pattern was to be tapped and the whole gone over again and again till the signal to stop was given. This made a series of repetitions of a cycle of forty digits, each made up of four inner cycles of ten digits, that had to be repeated four times before the subject was permitted by the

directions to return to the original cycle of ten and gain control of the entire series from the beginning once more. Two or three of the subjects found it necessary to return to the original series long before the whole 160 digits had been repeated, in order to get a 'fresh start.'

The records of the three subjects who worked regularly through the whole series of groups will be given here. Two of the others will be mentioned below, in noting the exhaustion phenomena. These three subjects had been over the original pattern on an average of twelve times each and were practically automatic so far as the sequence of movement was concerned. The conditions were kept as usual. The following table shows the individual differences and the changes that appear in the records of this group of tests.

Table I, 1.

Subjects	Rn.				Ch.		Sd.	
Order of Test	a	b	c	d	a	b	a	b
Errors, 1st min.	2	3	1	6	4	0	1	1
2d "	3	2	1	4	0	0	0	1
3d "	4	2	2	2	0	2	0	0
4th "	8	2	2	0	0	0	0	1
5th "	3	4	4	3	0	3	1	0
6th "	3	2	4	4	3	0	1	1
7th "	5	3	3	2	0	2	0	1
8th "	3	4	4	7	0	3	0	1
9th "	7	1	4	2	2	3	1	1
10th "	5	1	1	2	0	1	2	1
11th "	4	1	4	1	3	2	5	0
12th "	1	2	3	4	5	2	0	2
13th "	5	4	1	1	4	4	0	0
14th "	1	1	4	2	2	6	2	0
15th "	4	1	1	4	2	4	0	0
16th "	1	0	3	3	4	4	0	3
17th "	2	3	6	2	2	6	0	0
18th "	1	2	3	1	3	1	0	0
19th "	0	1	1	5	3	5	0	1
20th "	1	6		2	1	5	5	2
21st "	1						2	2
22d "							0	1
23d "							4	0

TABLE I, 2.

SUBJECTS	Rn.				Ch.		Sd.	
ORDER	a	b	c	d	a	b	a	b
1st period	85.2	98	118.4	136.5	78.8	90.6	75.4	92
2d "	78	83.6	119.4	123	81.6	89.4	72.2	93
3d "	81	92.2	112.4	112	86.4	95.6	77.2	92.8
4th "	86.6	95	96	99	97	98	79.5	96
5th "							86.3	106.8

TABLE I, 3.

SUBJECTS	Rn.				Ch.		Sd.	
ORDER	a	b	c	d	a	b	a	b
1st min	107	123	121	150	80	95	74	91
2d "	96	104	116	91 ?	60	93	90	103
3d "	83	90	100	—	88	91	67	94
4th "	78	77	121	133	78	90	75	90
5th "	62	96	134	127	88	84	71	82
6th "						97		

Table I, 1, 2, 3. '1' and '2' are similar to records in practice series; part '3' of the table is to show the changes in rate per minute for the first five minute period of each of the records given. In test 'b' made by subject 'Sd.' the errors for minutes 24, 25 and 26 are 2, 2, 1, respectively. These tables again show merely those points that are characteristic of the whole number of experiments taken. These records were selected to show the range of results and are not so typical as others that might have been selected.

Rates of tapping.—Part 3 of the table gives the rate of tapping per minute for the first five minutes, We find at once the distinct indications of the beginning 'spurt' or 'Antrieb,' which shows itself in the second minute. The second record of 'Sd.' where the 'Antrieb' was somewhat late in attaining its maximum still shows the characteristic decline immediately following. The recovery from the 'Antrieb' begins in some instances as early as the third minute and shows from there on the ordinary influence of 'Anregung.' Inside this general rise there is, however, an important secondary Antrieb. This follows a decline from the first Antrieb and is not due to the definite

establishment of the Anregung phase, but rather to a backward swing of the preceding wave. Such a fluctuation may be expected, when the subject is permitted to determine a rate satisfactory to himself. It is therefore of considerable interest in analyzing the formation of the Anregung phase of the work curve. The appearance of a stable condition of Anregung is put off in these tests and in the later ones, till the beginning of the third five-minute period, so that when the rate is averaged by five-minute periods the Antrieb covers the period of the first five minutes, the preliminary Antrieb and the return wave being obscured and bunched as a single Antrieb.

Record four made by 'Rn.' fails to show any recovery that continues throughout the test. Record three made by the same subject shows the recovery but again shows a decline that lasts during the remainder of the test.[1]

Errors.—There appears in the errors something we have not noticed in previous experiments, and had not expected to meet with in these. *The errors per minute show but little if any tendency to drop in number under the influence of practice or to increase through any possible fatigue.* We can only call attention to this point at this place as the tests are not arranged to bring out the reciprocal effects of these two influences. It is impossible to tell whether in this form of experiment, the opposing effects of fatigue and practice,—assuming again that Kraepelin and others are right when they so describe them,—are equal in their influence on the number of errors. Fatigue to be sure has combined with it here the effects of ennui and monotony.

The second point worthy of note is the manifest tendency of the errors to appear in groups. This first came to our attention when the records were transferred to the columns in which the digits were added and the errors marked in red ink. The phenomenon had been noted and the final set of tests made and read when we were gratified to note that the same general

[1] In record four this subject reported drinking two cups of coffee just prior to the experiment, a stimulant that always arouses her to a high grade of mental excitement. The beginning rate is much higher than was natural and could not be maintained.

phenomenon had been found in connection with some practice tests made on the typewriter by Mr. William Schuyler (81a). The mention is but an incidental one and has not been followed up by those reporting it so far as the writer knows. Counting the errors by groups, every record shows this tendency except possibly the more automatic ones and those in which for other reasons the errors were kept down to a point where they were very few in number. Even in such records, when the character of the rate and other modifying conditions are taken into account, the position of the errors made is easily accounted for on the group principle.[1]

In the arrangement of errors as they appear in the earlier records of each subject the grouping is somewhat obscured by what may be called 'accidental errors,' a term that we shall attempt to justify further on. But even here the errors fall into small groups. The most noticeable individual difference appears in the number of errors and their distribution. In subject 'Rn.' the errors tend to decrease slightly toward the close of the test and throughout they show the grouping clearly. In subjects 'Ch.' and 'Sd.' a tendency to increase the total of errors per minute is apparent.

We have given much space to these preliminary tests, since it was here that the facts came out which led to further experimentation. Certain of the results can be shown here as well as in the later experiments. We are thus able to offer the later tests as practically corroborative in respect to these points. Mention was made above of two other subjects who were tried on these preliminary tests, but the records were to be given a separate discussion, on account of certain special facts that appeared. The discussion of those experiments will be taken up at this point, and the further analysis of the records and accompanying introspections will be referred to again and there correlated with the later material.

[1] The difficulty of making a separate record of these groups and their relation to the minute periods was so great, that it was considered best to transcribe them according to minute periods. This method gives us the chance to determine any possible relation the groups might have to the time record and also to the rate of the tapping curve. It does not materially obscure the actual error groups as they appear on the record irrespective of the minute periods.

c. Tests in which Exhaustion was Prominent.

In the simple series 4, 4, 2, 1, etc., subject 'Dy.' worked in the first practice test about fifteen minutes, reporting little or no fatigue, practically none in the region of the lip. The records were made by the lip movement described above. In the second series a week later, the digits 4, 3, 2, 1, were tapped. This simple series was given to allow the subject freedom from the disturbing influence of mental errors. After spending seven minutes in preliminary practice with numerous breaks and pauses, Dy. started in a natural, unemotional manner and continued for eight minutes when she stopped through exhaustion. This exhaustion was reported as general and it could not be located as specific fatigue of any one group of muscles or of any single organ in the body. It was expressed in psychical terms as a lack of any sort of imagery. The series of digits simply disappeared.

Where the taps could be counted the rates are about as follows: for second minute, 59 taps; seventh, 70; ninth, 70; thirteenth, 73; and for the thirty-six seconds just preceding the breakdown 53 taps were recorded, a rate per minute of 91 taps. There are very few errors throughout, on account of the simplicity of the pattern tapped. The breathing rate was as follows: 24.8, 25.2, 24, 22, 24, 22, 23, 22, 21.5, 23, 20, 18, per minute, showing a marked tendency to fluctuate and a gradual slowing to the end of the series. In addition to these changes, the average amplitude of the 48 respirations preceding the 'breakdown' is slightly more than twice as large as the average of the same number of respirations selected from the early part of the series. While becoming slower in the whole respiratory cycle, the respirations have distinctly deepened in both inspiratory and expiratory phases. The expiratory phase shows an increasing tendency to slight but distinctly marked irregularities.

The complex series was tried in two tests with this subject. In the first trial with the complicated series both the rate and the number of errors were kept practically constant till the break came. The rate per minute is as follows: 104, 100,

118, 110, 117, 112, 102, 110. The errors run 4, 6, 6, 6, 6, 5, 2, 5,—the drop in the seventh is due to repeating three of the simpler groups together by taking them out of their usual order. The rate of breathing runs as follows: 21, 21.5, 24, 20, 24, 19, 20.5, 18. This is an uncommonly high variation in rate and is practically the only indication on the objective side that the work was not well in hand. Introspectively, Dy. reported that she had just thought how well the whole tapping was going when suddenly and without warning the visual image of the series was lost and everything became blank, with no power to recover the imagery and continue the process. No muscular fatigue could be localized, but the heaviness that came with returning imagery of the situation was felt about the head. In the second test, 'Dy.' broke down completely at the end of fifteen minutes. She reported a

"general tired feeling with considerable nervousness, a very noticeable tension throughout the body, a feeling of increased heart beat," and, as she stated it at the time, "all the phenomena accompanying a totally 'gone' feeling and lack of muscular control."

Another subject, 'Fl.,' succeeded in passing through the practice series but broke down repeatedly on the more extended test. The general description follows that given above in all essentials, though the introspections vary characteristically. A large amount of eye strain was noticed, and in the five tests this localization of strain and tension was reported first; the maintenance of the eye strain is reported as finally giving 'a general feeling of weariness.' In starting any single test, she reports a considerable tension about the head, neck, shoulders, and arms; this was gradually replaced by a relaxation that was irregular in order of disappearance, except that the strain of the forehead throughout the single experiment was replaced in attention at irregular intervals by the 'general feeling of weariness' mentioned above.

The results of these preliminary tests have been reported at length here since they indicate what becomes in many of the subjects, with this type of test, a frequent occurrence when any attempt is made to 'speed' them. We shall refer to the

descriptions given here again in connection with the 'rapid series.' *These subjects were among the most skilful introspectionists of the group, yet their total failures come without warning and at times when introspection and the whole 'feel' of the process reported the work satisfactorly in progress. Exhaustion seems to be the result of a practically complete focalization of all available energy on the continuance of a single function.*

d. Group II. Results.

Rate of tapping.—The results of the experiments in group II will now be taken up. The majority of the rate records reported will be given on the basis of the number of taps per minute and per five-minute groups. A number of these records, however, present the same phenomena when counted in a slightly different manner and, when grouped by this latter method, are more easily correlated with the error groups; hence we shall mention certain results on this basis first.

This rate is determined by taking the total time necessary to record a single series of ten digits and its accompanying calculations, i. e., the time spent in repeating or tapping each 'cycle' of digits. The method[1] is inapplicable except in a few of the series that are freest from unambiguous errors and are entirely continuous. This method of dealing with the records, where used, finds results comparable to curves showing increasing automaticity, decrease in the time of doing any regular piece of work through practice, and all factors that go with the transition of a memory task, as it passes from the early stage

[1] One reason why this method of assembling the results need not be used throughout (when the rates are also determined on the scale of an arbitrary unit, as the minute or the two or five minute period) is that the two times will vary in parallel. A few determinations will show just how many of the arbitrary units, minute periods, approximate in length the actual average time spent in making the repetition of the whole problem or 'cycle,' and this average time when applied to the assembling of the rates will show the fluctuations that appear in the time spent in repeating this common unit of the series. As a matter of fact this precaution, even, is not necessary in order to show whether the practice gains and fatigue effects are opposing influences. Practically any grouping, from one minute up to five, shows changes parallel to the larger fluctuations in the time spent in repeating the common unit or cycle of the series.

of instability to practically automatic associations and mere 'cue' reactions.

The results in those series that could be counted thus are very interesting. They show, first, that the condition used, i. e., allowing the subject to select his own rate of speed, is as regular in the results produced as is the more usual one of insisting on the greatest possible rapidity. The decrease in time necessary to repeat all the digits memorized and to make the calculations is a fairly regular one, and where the actual time can be determined with accuracy the records show no leaps so large as to indicate that the subject is not in as perfect control of the process as if he were working at a maximum speed. The introspections that accompany the change in conditions are indicative of this. In the slow series the subjects arranged the mental speed to suit a rate of the lip that was easiest and readiest in getting the best results for any period of time. This means, of course, that they were not working as rapidly as in cases where they were asked to attain their speed limit, but it also means that the coördination of lip movement with memory images was a much closer one. All of the subjects when put on the rapid series become conscious immediately that they could go much faster if they could only make the lips move as rapidly as they saw, heard, or 'felt' the digits. It was necessary all the time to keep the mental 'movement' back in order to prevent tetanus and consequent confusion through the lip movements.

The following tables and curves will show the main results, and the comparisons that can be instituted at this point in the experiments.

Table II, a gives the number of seconds required to make the calculations and to repeat the entire forty digits constituting the cycle, or to make the 143 taps necessary in series 'B–1'[1] and the 130 of 'B–2.' The consecutive numbers at the left indicate the number of repetitions made in each case. The totals of the columns of seconds will give the number of seconds used in the several experiments. These are given for the con-

[1] See page 55 for meaning of 'series B–1,' etc.

TABLE II, a.

Series	B – 1					C D F			B – 2						Rapid Series, D.	
Subjects	Ch.	Rn.	Sd.	Pn.	Ym.		Ym.		Ch.	Rn.	Sd.	Pn.	Ym.	'A.'	Ch.	Ym.
Time of Series in Min.	25.5	20	27	25	23.5	16	21	28¾	22.5	20	25.5	21	23	21	20	19.5
Cycle 1	120	138	150	85	90	103	150	145	120	139	105	123	82	108	64	64
" 2	146	124	120	115	92	97	141	131	130	159	100	110	99	117	61	68
" 3	107	120	127	125	99	94	133	120	153	153	—	118	89	98	64	65
" 4	107	120	119	122	83	85	132	120	?96	?135	101	103	?71	89	59	69
" 5	120	128	117	119	84	90	137	108	123	152	99	101	98	89	58	59
" 6	110	107	109	?122	?42	78	137	117	119	150	76	110	94	86	69	60
" 7	112	115	101	132	?91	75	134	?72	127	147	94	119	95	83	63	58
" 8	110	100	117	129	84	80	129	108	?92	142	98	?101	87	84	68	?38
" 9	109	110	134	125	98	78	129	95	116		101	131	?59	80	?61	54
" 10	118	100	148	116	89	87		122	122		101	134	—	86	66	63
" 11	120		109	120	92	78		112	116		103		87	86	?56	62
" 12	123		106	116	81			101			91		81	83	60	59
" 13	110		113		78			99			82		?63	85	78	56
" 14					79			104			86		90	82	72	57
" 15					80			101			89		86		77	53
" 16					85						92		?60		56	54
" 17															61	53
" 18															62	54
" 19																57

venience of the reader at the top of each column, in minutes. The C, D, F, group assembles all the series of subject 'Ym.' which could be handled in this manner. Where omissions in the columns occur, the group was broken up in the record in such a way that it could not be determined exactly how much time ought to be counted as spent on the 143 or 130 digits though the actual total time could be easily determined by subtraction; since this was of little value without the size of the groups repeated or omitted, it is not given. Other blanks in the records or uncertainties of any kind are indicated by question marks placed with the number about which the uncertainty lingers.

All of the curves show the ordinary results of practice.

Practice Changes.—Outside a certain variation[1] due to the character of the task set, and entirely independent of it as is shown by the changes in the curve in the method here used, is a different grouping of the practice changes. For subject 'A.' the rise and fall in the curve is less prominent, because of two things. The practice series that had preceded this series had been but two in number as against the eight to ten of the others. Secondly, only one of the group could be evaluated in this manner, hence a lack of material. The results in the curves on the basis of minute periods are much like the groupings here shown. *The peculiar thing about the curves that was noticed first in the errors, and here only secondarily, is the definite decrease in the amount of work done or in the accuracy of that work at certain particular points in the single experiment.* Though they cannot be foretold to the minute, the general nature of the groups that will appear and where they will fall, remain singularly constant for the same type of work.

[1] Practice changes are partly masked by the different degrees of difficulty presented by the work itself. For example, the repetition of the first ten in every cycle in the majority of the tests is that of a simple series of digits without any calculation. The second ten has calculation, and is thereby increased in size, so that when finally completed more actual taps have been made than in the first. This gives to the majority of the curves based on the amounts tapped per minute, a form that is slightly extraneous to the actual fatigue and practice effects of continuous work. The curves of group I, and 'A' of group II, show what the regular curve is as compared with those of the latter series.

Certain individual differences appear. In subject 'Pn.'s' records, the groups are larger and more noticeable toward the end of the series. These 'falls' are independent of the general rise or fall of the practice or fatigue curve, being superimposed upon it. In subject 'Ch.' they are more decided in character at the beginning and toward the close, control over the experiment being apparently better during the minutes ten to fourteen. With the exception of the initial drop of series 'B,' subject 'Rn.' shows the more steady portion of the series in the first eight or nine minutes, and after that the usual periodicity in rate. Subject 'Sd.' exhibits periods of decline in rate that are much slower in culmination than in any of the other subjects. The groups in the records of 'Ym.' and 'A.' are more numerous.

A second table to show the mean variations and averages of the times required to repeat the unit of the experiment follows. This is not intended as a mathematically accurate presentation, for the experiment and its data do not readily lend themselves to this kind of treatment.

The figures here given indicate somewhat more clearly than the original set of figures alone could do, that there are certain relations, frequently overlooked, existing between the parts of a single series of continuous mental operations. To show the variations for different subjects, the averages may be used to some advantage. The mean variations show slight increase in the 'at will' series over those which are done at the highest degree of effort applicable to this kind of work. Taking the plus and minus signs, we have the form of the practice curve excellently outlined. They also indicate the relative weight that can be assigned to practice after deductions are made, such as Anregung and Antrieb. They show further what is more important here, since general fatigue ought not to be very prominent in such short series, viz: the decided changes in rate of repetition that occur at fairly regular intervals. The discussion of individual variations will be omitted till the other results are enumerated.[1]

[1] It may be appropriate at this point to repeat what has been said in regard to the work of Voss on a point somewhat similar to the one that comes out

TABLE II, b.

SERIES	B – 1					C D F			B – 2						R. S. – D.	
SUBJECTS	Ch.	Rn.	Sd.	Pn.	Ym.		Ym.		Ch.	Rn.	Sd.	Pn.	Ym.	A.	Ch.	Ym.
AVERAGE	116.4	116.2	120.7	119	87	86	135.8	113	125	149	94.5	115	89.8	89.7	63	59.1
A. V.	7.5	9.8	10.1	7.1	5.6	7.5	4.64	73.5	7.7	5.3	7	10	4.9	7.7	48.3	4.08
Cycle 1	3.6	21.8	29.3	−34	3	17	14.2	32	−5	−10	10.5	8	−7.8	18.3	1	4.9
" 2	29.6	7.8	−0.7	−4	5	11	5.2	18	5	10	5.5	−5	9.2	27.3	−2	8.9
" 3	−9.4	3.8	6.3	6	12	8	−2.8	7	28	4	?	3	0.8	8.3	1	5.9
" 4	−9.4	3.8	−1.7	3	−4	−1	−3.8	7	?	?	6.5	−12	?	−0.7	−4	9.9
" 5	3.6	11.8	−3.7	0	−3	4	1.2	−5	−2	3	4.5	−14	8.2	−0.7	−5	−0.1
" 6	−6.4	−9.2	−11.7	3	4	−8	1.2	4	−6	1	−18.5	−5	4.2	−3.7	6	0.9
" 7	−4.4	−1.2	−19.7	13	−3	−11	−1.8	?	2	−2	−0.5	4	5.2	−6.7	0	−1.1
" 8	−6.4	−16.2	−3.7	10	11	−6	−6.8	−5	?	7	3.5	−14	−2.8	−5.7	5	?
" 9	−7.4	−6.2	13.3	6	2	−8	−6.8	−18	−9		6.5	16	?	−9.7	−2	−5.1
" 10	1.6	−16.2	27.3	−3	5	1		9	−3		6.5	19	?	−3.7	3	3.9
" 11	3.6		−11.7	1	−6	−8		−1	−9		8.5		−2.8	−3.7	−7?	2.9
" 12	6.6		−14.7	−3	−9			−12			−3.5		−8.8	−6.7	−3	−0.1
" 13	−6.4		−7.7		−8			−14			−12.5		?	−4.7	15	−3.1
" 14					−7			−9			−8.5		0.2	−7.7	9	−2.1
" 15					−2			−12			−5.5		−3.8		14	−6.1
" 16											−2.5		?		−7	−5.1
" 17															−2	−6.1
" 18															−1	−5.1
" 19																−2.1

Tables II, b, and II, c. The averages in the horizontal column are simply the arithmetical averages of the time in seconds required to repeat the cycle of digits once. 'Cycle' in this connection means the ten digits that are memorized plus the calculations that are imposed upon them at each repetition. The average variation is given to indicate with some degree of accuracy the amount of fluctuation that an 'at will' rate will occasion, and may be compared with the same in the rapid series given at the close of the table.

TABLE II, c.

SUBJECT	RATE CHANGE AS LOCATED IN.	SERIES B – 1.				SERIES B – 2.				
A.....	Cycle					2		10–11		
	Min. period					3–4		15–16		
Pn....	Cycle	slight		**6–7**	11		3	**7**	**9–10**	
	Min. period	decrease		12–14	22		5–6	13	17–19	
Sd....	Cycle		3		**8–9**		**4**	9–10		**15–16**
	Min. period		6		16–19		5	13–16		23–24
Rn ...	Cycle		**5**	**7**	**9**	**2**		**5–6**		
	Min. period		9–10	13	18–19	3–4		11–13		
Ch ...	Cycle	slight	**5**		**10–12**		3	**7**	**10**	
	Min. period	decrease	9–10		19–23		6	14–15	19–21	
Ym...	Cycle	3	7	**9**		2	5	**9–10**	13–14	
	Min. period	4–5	9	12–13		2	6–7	13–14	18–20	

Table II, c, shows where the distinct decreases in the rate of tapping appear. Those in dark-faced type are the more important of the rate changes; they are located in the whole test in two ways, first by the 'cycle' and secondly by the minute period where the change occurs.

So far our results with one important exception are entirely comparable with those of other investigators. We have found practically all the individual variations that have come out in earlier experiments. The practice curve in its various forms occurs ranging from the curve showing continuous practice effects during the time spent in the test to the opposite curve of continuous fatigue, the negative practice curve.[1] The curves show distinct evidences of Anregung, Antrieb, Ermüdungsantrieb.

We attempted, by careful questioning, to determine the subjective nature of the rate curve during these experiments through the introspections of the individual subjects. In no case in the 'at will' tests could we elicit any significant answer to our question about changes in the rate where positive introspections were obtained. The records show that there was as much chance of an opposite change actually having taken place as the one assigned by introspection. Some of the subjects even went so far in one or two instances as to state that they were

here, though the main similarity comes to the front in the terms he has seen fit to apply to the variations that he found in mental work, rather than in the nature of the results themselves. Using the additions of the Kraepelin method of experimentation and measuring the length of time necessary to make each separate addition he found an exceedingly regular series of fluctuations. The 'mode' of the curve fell at lengths of 13/5 seconds, and the sides of the curve fell off in the following frequencies: 7/5, 10/5, 16/5, 19/5, 23/5 seconds. These are, according to the author, entirely comparable to the fluctuations of attention found by many other investigators in the field of sensations. He concludes that he is dealing with the same phenomenon, and explains it as a central one. In so far as the experiments we have carried out are amenable to this method of treatment our results agree with astonishing accuracy with these details published by Voss. The work we have set our subjects is much more difficult, but otherwise the results are similar. Voss made no attempt to evaluate the larger fluctuations that we mention as the most characteristic thing in our curves. *A priori*, we would expect to find them smaller in the simpler work; he merely mentions that such fluctuations were found in rate and that they seemed to show no regularity. We shall find the same thing true in the method of handling the errors in the more homogeneous type of work set by these experimenters. Their work and method of experimenting do not bring out the larger changes in attention which may be correlated with 'synaptic' fatigue.

[1] To be sure, this is merely a happy accident as far as the experiment planned is concerned; it could not be foretold that such was going to be the case in the selection of the subjects.

conscious of following the motor in its regular whirr, although the records in these two or three cases are particularly definite regarding the fatigue impulse. Other changes, as the lessening of calculation pauses, averaging up the intervals preceding easy and difficult calculations, and the like, correspond without need of comment to such results as those of Voss and others.

The one exception noted in our results we find in the distinct groups that occur, which we have described above. The complexity of the work is, no doubt, partly responsible for their appearance. Still they have been clear enough for previous mention by Voss and, in a number of the tests carried out on school children, their prominence has been noted, but no attempt has been made actually to describe them. We are undoubtedly dealing here with the 'Ermüdungsantrieb' that Kraepelin has described and named as the spurt that comes near the end of a decrease in the rate of work. The curves here shown, after the practice phases of the curve are taken into account, indicate that the time spent in the decline is always longer than the time spent in the recovery, hence Kraepelin is justified in calling such rapid increases in the rate 'Ermüdungsantrieb.' *But the significant part of the curve for fatigue is the slow decline of from two to three minutes in length that occurs at fairly regular periods.* Kraepelin has mentioned but one variation of this type, that one which precedes the declining portion of the work curve and comes, in the two hour test, at a period ranging from 70 to 90 minutes after the beginning of the experiment. The numerous Störungsantriebe that, so far as introspection and the form of the record are concerned are entirely similar to the 'Ermüdungsantrieb,' Kraepelin considers too irregular for evaluation.

It is possible to find the changes of which we speak in those records that were done at the limit of speed, though they are lessened in degree as may be expected. With work that is much simpler than our own and more mechanical in its method of record, it seems possible that these fluctuations will almost entirely disappear from records as gross as these. The work on muscular strains and the relations of their regularity to the relative weight of the loads lifted indicates what we mean by

work tests where fluctuations disappear. A point has been reached where practice as well as fatigue disappears, and this point constitutes the zero point for the coördination of the individual with reference to a specific kind of work, mental or muscular. In other words, the habituation process goes on below the level of objective fatigue.

Errors.—In the tables that follow, the results of the experiment are presented from the standpoint of the errors that appeared. Their grouping, their increase or decrease in a single record, and their possible classification, will be discussed.

The absolute number of errors increases very slightly in some records, from one minute to the next; the reverse statement is, however, apparent in others. There is an initial group of errors due to lack of practice. This initial group soon drops to a minimum or to no errors per minute. From this point on the errors in some records gradually increase in number. This is true of a majority of the tests. Certain records, however, present an increase after this initial group, then a slight decrease.

The interesting fact that appears in regard to the errors, however, is the fluctuating character of the totals per minute. This has occurred in every series so far recorded. In any one subject the errors tend to group themselves. These groups occur at more or less regular intervals for a single subject. The length of time between the intervals varies apparently with the amount of practice and the degree of difficulty of the particular series. In some instances, instead of the actual number of the errors in any group increasing, they may slightly decrease. But as long as errors appear in the record they tend to group themselves. In other words, the errors, which occur after the first gathering of the sensory controls that are necessary in order that the process may go on at all, are more or less constant and independent of the real results of practice.

Grouping in 'practiced 'series.—After the errors begin to appear, even in a practiced series, that is, when the smoothness of the mental 'coördinatings' is overcome by the preliminary stage of fatigue, then grouping appears in the manner described and it is in no specific sense different from that form in which

Table III, a.

Subject	Ch.							Sd.						
Series	A	B – 1	B – 2	C	D	E	F	A	B – 1	B – 2	C	D	E	F
Min. 1	0	2	0	0	0	0	2	0	1	0	0	0	0	0
" 2	0	4	3	0	0	0	1	0	1	0	0	0	0	2
" 3	0	2	0	0	1	0	0	0	2	0	1	1	0	0
" 4	1	0	0	0	2	0	1	0	1	5	0	0	2	1
" 5	0	1	0	0	2	0	1	1	1	1	0	1	0	0
" 6	2	0	0	0	3	1	1	0	2	0	1	4	1	0
" 7	0	2	0	0	2	0	1	0	3	3	1	3	1	0
" 8	0	2	2	0	2	2	0	0	1	0	0	2	2	0
" 9	0	0	3	1	3	3	1	0	0	1	2	1	2	1
" 10	1	0	0	0	0	0	1	0	0	0	5	1	2	1
" 11	0	2	0	1	1	0	0	0	1	0	0	0	5	0
" 12	0	0	1	1	2	2	1	0	1	1	1	2	1	0
" 13	0	2	1	0	2	1	1	2	3	3	1	0	1	0
" 14	2	3	0	1	2	0	1	0	0	1	0	0	2	1
" 15	2	3	2	0	1	0	2	0	4	0	3	3	4	1
" 16	0	0	1	1	0	0	0	1	1	4	1	2	3	0
" 17	1	0	1	3	0	0	0	0	2	5	0	0	2	0
" 18	2	1	1	0	0	0	0	0	1	4	3	3	1	6
" 19	1	1	0	0	0	0	0	0	2	1	2	1	4	0
" 20	1	0	1	0	0	3	0	0	4	0	1	1	4	0
" 21	0	3	0	0	0		0	6	1	1	1	3	3	0
" 22	1	0	3	0	0		1		1	2	2	0	2	2
" 23	0	1		0	0		0		2	1	4	1	3	2
" 24		2		1	0		0		2	2	3	4	2	0
" 25		0		0	0		0		5	0	4	3	5	1
" 26				3	0		1		3					
" 27									0					
" 28														

Table III, a, is a detail table taken from the work of two subjects. In general they represent records that indicate an average grouping or one slightly below the average. Cross lines in the column indicate where the word 'change' spoken by the operator introduced the new part of the record; see p. 56.

it showed itself in the new series or any repetition of the series. The use of the new or 'practiced' coördination within the working period of a test, as in series 'D,' etc., is a decided relief as far as the making of errors is concerned. The relief is also evident when a new series is followed by another series

Tabl

SUBJECT	A.							Pn.							Sd.			
SERIES	A	B–1	B–2	C	D	E	F	A	B–1	B–2	C	D	E	F	A	B–1	B–2	C
Min. 1.........			5			3												
" 2.........							4											
" 3.........	5	5		5	15?											4		1
" 4.........									2	4	5	5	2	4			6	
" 5.........											3				1			
" 6.........				6			7							14?				
" 7.........																6	4	
" 8.........	3				3				5	7		5						
" 9.........							2			7			3	5				
" 10.........																		7
" 11.........		5	5			3?						4						
" 12.........				4									5	4				
" 13.........	8						4		2		5				2	5	5	
" 14.........			4											5				
" 15.........	9			3							3	4	5			7		4
" 16.........						2?									1			
" 17.........										5				6			13	
" 18.........					3							4	4					6
" 19.........			3				4	3			4			4				
" 20.........																7		
" 21.........															6			
" 22.........																	5	
" 23																		9
" 24.........																		
" 25.........	...															10		
" 26.........																		

Table III, b. This table gives in totals the errors of each test for the six subjects wh
made by subject A, series 'A'. The errors did not occur exactly as shown but are massed
containing the largest number of errors. The minute periods before and after this one alwa
Turning to Table IV, will enable one to test the validity of the grouping shown here.
apparent. Reference may be made to pp. 56, 57 to determine the nature of each of the seri

Table III, b.

Sd.					Rn.							Ch.							Ym.						
-2	C	D	E	F	A	B-1	B-2	C	D	E	F	A	B-1	B-2	C	D	E	F	A	B-1	B-2	C	D	E	F
																					5	3			
				2		5				3			8	3				3		5	4		3	3	
	1								2										4		4				19?
6			2				6				5										4	8	6		
						3														12	3				
		9							2			3				7		4			5		5	3	
4						5							4						5		6				
			5								6										3				9
				2		5	7		2					5		7	5		6	8	1	23	12		
	7				2	5					6										5				
			8							2			2		3						2				
		2							2		9						3				3			3	
5					2			6								6			10	8	8				14
				2			2		2				8								5	24	12		
	4	5	9			6				2		4		3				4			4				
								8													1			5	
13					6	3					8				4					7	7	4	4		14
	6	4		6								4									4			3	
			9				5			5											5				
								2									3			10	2		10		
		4	5		3		6				6		4						4		4				
5				4										4				1			2				
	9																			7	4				
		8	10										3												
															4			1							

bjects who made the entire series of tests. We may illustrate the arrangement by reference to the errors
massed at distribution points. The place where the figure is min. 3, 8, 13, etc., denotes the minute
one always contain less errors. A comparison of this table and Table IV, a will show how it is made up.
wn here. The errors of Ym. for test B-2 are given in full to show the series in which grouping is least
f the series marked A, B-1, etc.

that is new, or a highly practiced series is followed by one that is new. The relief here cannot always be determined from the record because of the demand for new coördinations.

Arrangement of groups.—The distance between any two groups is more or less constant for different places in the series. There are seldom long periods wholly free from errors. This distance is practically always greater between the initial group and the first group. (i. e., the first group that seems to be definitely due to the effects of fatigue and the wanderings of attention) than it is between any of the groups that follow. The instances where this is not exactly true will be discussed separately.

There is no general tendency for the errors to increase in their absolute number as the work of the series progresses beyond the first fifteen or sixteen minutes of the series. Exhaustion or sleep is the new phenomenon that appears rather than any quantitative change. As stated above there is in the majority of the single experiments no rate change in this short experiment that would indicate the rise of general or lasting fatigue.[1]

Tables of Grouping.—It is somewhat difficult to determine just the manner in which these groups in their details ought to be measured, either to show that they are of less importance than they at first appear to be, or to bring out their real nature with respect to the other characteristics of the work curve. In order that there shall be as little danger as possible of 'forcing' any of the changes or of covering up others that ought to be brought out, we shall try several methods of assembling the results. The first in order of presentation is that which we have mentioned, (Table III, a), (1) the simple arrangement of the errors as they occur by minute periods in each continuous series. After that we shall (2) group in order the errors as they appear to arrange themselves around certain points in the

[1] In this connection it seems advisable to state again that there is no intention to institute a comparison beyond the more general features between these records and the two-hour and longer records of other investigators. What would happen to our curves if arranged with that end in view is in its general features undoubtedly what has been pointed out before, but we should expect to find accompanying those phenomena the others just mentioned.

series (Table III, b), and (3) as they appear when brought together by two, three, four, and five minute periods (Table III, c and III, d). The actual distribution in the tests, which is practically similar to the minute periods, is omitted.

TABLE III, c.

	TOTALS BY TWO MINUTE PERIODS.							TOTALS BY THREE MINUTE PERIODS.							
SERIES.	A	B−1	B−2	C	D	E	F	A	B−1	B−2	C	D	E	F	
Min. 2.....	0	6	3	0	0	0	3								
								0	8	3	0	1	0	3	3 Min.
" 4.....	1	2	0	0	3	0	1								
" 6.....	2	1	0	0	5	1	2	3	1	0	0	7	1	3	6 "
" 8.....	0	4	2	0	4	2	1								
								0	4	5	1	7	5	2	9 "
" 10.....	1	0	3	1	3	3	2								
" 12.....	0	2	1	2	3	2	1	1	2	1	2	3	2	2	12 "
" 14.....	2	5	1	1	4	1	2								
								4	8	3	1	5	1	4	15 "
" 16.....	2	3	3	1	1	0	2								
" 18.....	3	1	2	3	0	0	0	3	1	3	4	0	0	0	18 "
" 20.....	2	1	1	0	0	3	0								
								2	4	1	0	0	3	0	21 "
" 22.....	1	3	3	0	0		1								

Table III, c. The above table exhibits the errors as they occur in the tests made by subject Ch. by two and three minute groups. These details are given as representative of the tests for all the subjects and may be compared with those tables where the errors are grouped according to their apparent distribution as in the preceding table.

Location and number of groups.—We find an initial group in practically all the series. There are two exceptions to this. The first is in those series, marked 'A,' where the subjects had had a preliminary practice series on the actual ten digits used here, of eight to ten days. The second is where the material given proved to be less difficult for the subject than expected, because of some special method of learning the material. *This first group appears within the first three minutes of the test, and with an exception to be noted below, at the end of the sixth minute is approximately over.* This initial group is

TABLE III, d.

	PERIODS.	TWO-MINUTE GROUPS.						PERIODS.	THREE-MINUTE GROUPS.						PERIODS.	FOUR-MINUTE GROUPS.						PERIODS.	FIVE-MINUTE GROUPS.					
SUBJECTS.		A.	Pn.	Sd.	Rn.	Ch.	Ym.		A.	Pn.	Sd.	Rn.	Ch.	Ym.		A.	Pn.	Sd.	Rn.	Ch.	Ym.		A.	Pn.	Sd.	Rn.	Ch.	Ym.
	2	25	13	4	12	12	34																					
								3	43	23	8	22	15	50														
	4	21	15	12	9	7	35								4	47	28	19	27	19	69							
																						5	48	39	21	32	23	79
	6	16	20	11	11	11	34	6	20	25	21	17	15	53														
	8	8	18	14	17	13	37								8	24	40	28	32	24	72							
								9	14	26	23	28	24	60														
	10	9	13	9	17	13	45															10	28	42	40	44	34	88
	12	20	19	11	17	11	33	12	22	26	21	27	13	55	12	29	32	28	36	24	77							
	14	15	15	13	18	16	51																					
								15	26	21	29	31	26	67								15	47	40	41	49	38	90
	16	13	15	23	18	12	35								16	30	29	41	39	28	86							
	18		16	24	13	9	34	18		25	39	21	11	'53'														
	20		7	17	16	7	'23'								20		23	47	29	16	'57'	20		34	65	36	18	'69'
								21		'8'	35	19	10	'42'														
	22			21		8	'29'																					
	24			'19'		4	'17'	24			35		'9'	'27'	24			50		'12'	'46'							

Table III, d. This table gives the *total* errors for six subjects for the seven tests constituting group two (II) of the experiments. By reading down the columns any increase or decrease in number of errors may be readily noted. The 'Periods' are serial; the numbers below the word 'Period' merely indicate the last minute of that period.

undoubtedly due to the unpracticed character of the work and the newness of the calculations to be made.

After the first or initial practice group, the record usually shows a decided drop to no, or relatively few errors, till a point approximately covered by the period from the ninth to the eleventh minutes. This second group is pushed on by the initial period coming farther along in the series, when it may fall as late as the twelve to fifteen minute period. The time and place in the series are not absolutely determinable but the groups are there in every case. An extension of the group from the sixth to the tenth minutes would include all of the error changes that are significant and seem to be due to causes other than those operating through lack of practice and the beginning inertia.

We find this second group sometimes coming at the beginning of the period mentioned, at other times at the close of it. If the first condition exists, then the third group will fail anywhere from ten to thirteen minutes inclusive. If the latter condition prevails, the third group is pushed on to the thirteen and fourteen minute period or even as far as the fifteen to sixteen minute period. In any case, where the grouping has appeared at all in the series in the places above described, a third group always appears by the close of the sixteenth minute and usually before this. This group as well as the second will be of especial interest, in that it is here that the indications of the physical and mental condition of the subject come to light most prominently.

When the experiment was carried on far enough, a fourth group appeared in the period ranging from seventeen to twenty-one minutes. This wide limit is given, not because any one series shows the group extending over this period, but because of the few experiments made. Usually this group has been the most definite and regular after the third that has appeared, when the series has been long enough to include it. It was somewhat unfortunate that they were not made of such a length as always to include this, but nothing of the sort was expected in the inception of the experiment.

The distance maintained between these groups is subject to

constant fluctuations; which, to the writer, indicates the really psychological significance of the groups that do appear. The groups are too gross and too irregular to indicate any closer correlation with the fluctuations that are commonly assigned to the attention process and probably they show none at all. Their origin and place in the series must be explained on other grounds.

This grouping of errors holds true even after the practice has been carried to a high degree of automaticity. Groups of the later orders could be found even after the eighth and ninth days, in the practice series. These correspond fairly well with the periods of the less practiced tests when the changes due to practice are considered. It is found that retention of practice (Uebungsspur) will affect this type of error and work though in much less degree than is popularly supposed. Swift (81a) has shown what some of these experiments indicate, that the errors remain constant or even increase while daily practice effects are still going on. This increase in errors is, however, a common phenomenon in tests on school children and appears to be independent of the practice change as that appears in an increase in the amount of work done. How the practice does finally affect the errors by reducing them, has never been discussed so far as the writer is aware.

Pivotal Groups.—If we discuss the errors quantitatively, we find one or two things that seem worthy of note. For example, group four of the errors in all the series shows a constant tendency to drop below, in the number of errors, the preceding group; i, e., *group three is practically always the largest group in the series.* This is a constant phenomenon if we except the results of the subject 'A.', and take the totals of the errors by five minute or four minute periods. The preliminary group in the case of 'A'.s' record shows the largest total of errors, due no doubt to the lack of preliminary practice. This is borne out by the practice curves in the records of this subject, which are very pronounced. Subject 'Pn.' shows a change that is quite parallel with his negative practice curve. His *largest group of errors*, counting by five minute periods still falls in the second group instead of the third as usual. This

change is interesting when we consider that he is the only subject of the number used, who presents a consistent negative practice curve.

In several instances, where the record was run up to twenty-eight and twenty-nine minutes before stopping, a fifth group appears, showing that the tendency to group is not lost after group four is over. Group five is best exemplified by the records of 'S.'

As far as can be determined, and the introspections will be discussed in this connection later, the number of errors and the distance between the groups, their size and length, their place in the series, all depend on the actual physiological condition of the subject as he begins the experiment, rather than on any particular degree of practice attained or on the difficulty of the work or on any shift in the conscious processes. Some few shifts noted above may be considered as exceptions to this statement: special imagery disturbances, external distracting stimuli, etc.; but they are exceptions which in no wise seem to affect the general statement here made.

When the errors appear constantly, or in much greater proportions than usual, the introspections tend to substantiate this by presenting some condition that could not be avoided except by interrupting the continuity of the series. For example, 'Ym.' shows the constant effects of a lowered physical condition in which he was compelled to record the series. 'Pn.' also shows the necessity for a particular type of control in the negative practice curve. His records were taken at the ebb in a difficult summer's work. The distance between groups of errors seems to be particularly instructive with regard to the general health under which the different subjects performed their tasks. Those working under poor health conditions show a very marked tendency to make the groupings shorter, more frequent and less clear cut, then the groupings that occur in the records that are made under favorable health conditions. As an illustration, the contrast between the groups in the records made by the two subjects 'Sd.' and 'Ym.' is extreme as regards the number of errors and the distance in time between the error groups.

Groups Two and Three.—A very interesting point appears in connection with the relation of groups two and three. The relation of these *two groups* is most clearly shown, though by no means obscured in other tables, when we count errors by periods of five minutes each. If we recall that group three is the largest group of the series, we see at once that group two is usually next in size. Not only is it the next largest group, but it also bears much closer relationship to the following group than is shown by any other pairing that can be made. It pushes into it and modifies it continually in such ways that the third group could not be described without reference to this first appearance of the errors after practice control had been gained over the series. Where a group disappears, it is one of these, or a coalescence of the two.

Evidence of the great fatigue importance of this point is offered in the high speed series that were instituted as a control over the regular experiments. Two of the subjects, neither of whom had had any difficulty with the other series, when asked to try this, were compelled to stop at the end of eight and a half or nine minutes, through tetanus of the lip and especially a general loss of muscular control. In the subject 'Ch.' tetanus was the principal annoyance; there was no control to 'show how and which digits had been counted.' Quoting further from the introspections, 'Ch.' says that "toward the close, sympathetic movements of the fingers and toes and other parts of the body were noticed." 'Rn.' succeeded in passing this point on the first record by stopping at intervals, but on the second day's work had to stop entirely because of the general loss of muscular control. This was shown by involuntary jerking movements of the arms, legs, and smaller paired muscles throughout the body, there being no apparent favor shown any one muscular group over another. 'Ch.' reported that after she had stopped, the lip and surrounding muscles seemed in better condition to continue than a great many other muscles that were affected.

The first two subjects mentioned above, whose records are reported as exhaustion groups, in two of the experiments failed twice each to carry the series over the nine-minute period.

The last test with 'Dy.' left her in such a condition that no more experiments were tried for some weeks.

Periods in which the subjects forgot the place in the series completely and had to be assisted by the experimenter, or stopped till the place was found, occur in the other records. These failures were well scattered and do not constitute anything distinctly comparable to the conditions above described.

Nature of errors.—An attempt to measure the character of the errors was made. This is largely unsatisfactory because of the reasons so often advanced against any such efforts. These reasons have been argued against the work of Sikorski, Ebbinghaus, Ritter, Burgerstein, and others, so only one or two main points will be restated.

In the first place, it is argued, it is impossible to locate the introspections on the record even if they be perfectly definite, and not interrupt the continuity of that record. Unless this is done there is no means of telling what the majority of the errors mean in mental terms, except where the coincidence is accidental. The calculation of what is an error and what value shall be given to the different types that can be distinguished in the records, are difficult points. This argument is complicated by the further observation that one error of the graver sort will have in its train several of less importance, whose actual importance is thus materially lessened. The question of including extra long periods of waiting between the separate groups as errors and causes of errors is pertinent, if one argue that all changes must be evaluated from the error standpoint.

In the tables appended, all of the observations mentioned operate, though we have given none a determinate value in measuring the number of errors. The reason for this appears to best advantage in the tables themselves. Everything that was not correct was marked an error in the tables first made from the records. These were used as the basis for our discussion concerning the grouping of errors, their increase or stability in number throughout the continuous series, and in the other discussions. In order to be sure that all this would not be vitiated by the possibility of one error constituting the

whole of the group phenomenon described, and for the other purposes here stated, this attempt to classify was made. The classification takes account therefore only of those errors that could be placed, and attempts to so define the types that there shall not be left a large number of errors unclassified. (The whole number of errors in these tables is equal to the whole number of errors found, so that all the errors are accounted for that are found in the discussion of their grouping, or that were marked as errors at any time.)

The following classification was followed throughout the study of the errors; (1) omissions of digits constituted one set of errors; (2) repetition, a second; (3) tapping more or less than the requisite number of strokes, where the more or less was only one stroke, constituted a third set; (4) mental uncertainty, i. e., the irregular form of tapping that could not be due wholly to an irregular lip movement; (5) splitting the digit into two smaller ones, practically the same mental error as the above, but entirely definite and clear as to what it means, viz: a pause in the mental process, then recovery, and finally finishing work consciously incomplete; (6) reversing two or more digits in a series, as 2, 4, for 4, 2, and 2, 3, 4, for 4, 3, 2; (7) errors in calculation, where they could be distinguished from class (3), the criterion being a digit that differed from the right one by two or more taps, as 6 for 4, or 7 for 9; (8) tapping a digit from another series than the one being tapped, where it could be distinguished from other errors, was counted as a separate group; this determination was possible only where a group of two or more digits was taken bodily from some other series, otherwise the error was assigned to the calculation and uncontrolled tapping classes. Error type (3) is divided into two parts in the tables, one for the surplus tapping and one for the failure to tap the complete number. The omission, repetition, and calculation classes were subdivided to show whether the error covered one digit, a group of digits within the series of ten, or the entire ten digits; for example, omissions were marked as follows: O_1, O_2, O_3, etc. (This table is not appended in all its detail.)

The first table constructed showed the character and number

of errors for each of the forty-two experiments in this part of the problem. These were then reduced to the figures representing the totals of each type per minute instead of the letters and characters used to designate the nature of the error and its place in the classification. This last table is the one given here.

Examining this table, we note that each type of error separately shows the distribution into groups and a distinct periodicity of a nature quite similar to the totals, but still differing enough to make it a class by itself. The totals of the entire sum of errors were then tried since this grouping appeared so well; and again, although the series used differ considerably in the arrangement of the experiments in several of the series, as well as in the changes made at irregular intervals in the series in some of the experiments, the totals are still found presenting a slight tendency to carry out the grouping first described. This is best illustrated by the average variations, or by a curve based on the totals of the fluctuations. To the writer this came as a distinct corroboration of the first table since it was not expected *a priori* on account of the great variability of the groups and the actual lack of practice effects of a positive sort in one subject.[1]

Two general classes of errors.—The errors as they are classsified seem to break up into two sorts, those that are due to so-called central causes, or purely psychical processes, as memory, vagueness of imagery, distinct stoppage of the thought process, etc., and those due to the more immediate sensory-motor process of recording. In the first class, we can easily put such errors as repetitions, omissions, reversing digits,

[1] Regarding the error groups of Table IV, it may be mentioned that the calculation errors are not so numerous as might be expected from the difficulty of the work. This may be due to two things: the difficulty of determining from the record what actually is a calculation error, and second, the way in which the experiments were arranged, i. e., giving the subject a chance to select his own rate which enabled him after the practice series to hold a rate that brought inside itself the work done on the calculations. The first cause undoubtedly operated in counting the errors and distributing them for subject 'A' into the divisions marked calculation and uncertainty errors respectively. We have put practically all the errors in the last division and have been unable to place any with certainty in the former divisions.

TABLE IV

TYPE OF ERRORS.	a. REPETITIONS.						b. OMISSIONS.						c. EXCESS IN NO. OF TAPS.						d. INSUFFICIENT NO. OF TAPS.						e. TAPPING		
SUBJECTS.	A.	Pn.	Sd.	Ym.	Rn.	Ch.	A.	Pn.	Sd.	Ym.	Rn.	Ch.	A.	Pn.	Sd.	Ym.	Rn.	Ch.	A.	Pn.	Sd.	Ym.	Rn.	Ch.	A.	Pn.	Sd.
Min. 1	1		2	1	1	2	3	1		1	2			1		7	1			2		4	2	1		1	
" 2	1						5		1	1	1	2	1	4		8	2	2				4					
" 3	1			1			1	2			1	1		2	1	6		1	2	1	1		2	1	1	1	2
" 4						1			1							8	1	2					1		1	1	
" 5		1				1					1	2	2	2	3	6	3	1	1	2	1		1	1	2	1	
" 6		1	1	2					2	1			3	1	2	6	4	4	1	1				2	2		2
" 7		1					1	1		3	1	1		4	5	8	2	1	1	3	2			1	2	1	1
" 8			1			1		2		1		2		2	2	10	2	4		1		2	2		5	1	
" 9	3		2	1						2	2	2			3	6	5					5	1	5	2	3	1
" 10		3	3		1		1		2	2	2		1	1	1	4	3	1		1	2	4			2	1	
" 11	1		3	1			2		2				2	3	1	12			1		1	1	1	1	2		
" 12	1	1	1	1	1		2	1		1	1	1	1		3	7	3	1	1	1			1	1	2		
" 13	3			1	1	1		1			1	1	1	2	1	7	8				4	3	1	3	1	2	
" 14			1	1	1	2	1			2		1		3	3	5	1	1	3	1	1	4	1	1	3	2	
" 15	2		2	2	1			1	3			1	2	1	3	8	2	2	1		1	3	2		5	3	
" 16	1	1			1	1		2	3	1		1	2	2	4	8	4					5	1	1	3	1	1
" 17		1	3	2	1		1		3	2	1			2	2	7	1	1				3		2		3	
" 18		4					2		2				1	2	5	5	1	2			2	1	2	2			1
" 19		1		1			1	1	6	1	1		3	1	1	5	2		2	1	2	2		2	2	1	
" 20				2	1			1			2	1				2		2		1						1	
" 21					1	1			7	1		1			10						3						
" 22		1	1			1						1			9	2						2					4

Table IV.

. TAPPING UNCERTAIN.					f. SPLITTING DIGITS.						g. REVERSED DIGITS.						h. CALCULATION ERRORS.						j. DIGITS FROM ANOTHER SERIES TAPPED.						TOTALS.	Av. & A. V.
Pn.	Sd.	Ym.	Rn.	Ch.	A.	Pn.	Sd.	Ym.	Rn.	Ch.	A.	Pn.	Sd.	Ym.	Rn.	Ch.	A.	Pn.	Sd.	Ym.	Rn.	Ch.	A.	Pn.	Sd.	Ym.	Rn.	Ch.		49.
1		2	1		2	1		1	2		1			1		1				8									53	+4
				1	1			2		3											1					1	1	1	43	−6
1	2	1	1		1	5	1		1											2						2			42	−7
1		1	1		3	1	4	2	2	1				1					1	1		1					1		36	−13
1						2	1	3	1					1															39	−10
	2			1	6?	4	2	3	1				1								1								54	+5
1	1		1			2	1	3					1							2	2	2							53	+4
1			2	1		2	2	1	1	1										3	4								55	+6
3	1	1	2	1		4		3		2								1			1								58	+9
1		1			2	2	1	4	1	1					1					3	3						1		55	+6
			2		1	1	1	2		1					1	1				3	3								50	+1
			1	1		2	1			2		1			1					2		1							44	−5
2				2				2	2				1						2										51	+2
2		1	1	3	1	2		2	2	1										4	1								56	+7
3			1	3		2	2	2		2				1						9		1							68	+19
1	1		1					1	2										2	1									50	+1
3				1		2		5	2			1								3						1			50	+1
	1		3				2	2		2					1				1	5									48	−1
1					1				1										2	2						2	1		45	−4
1				2		1		3	3					1	1					1	1									
				3				1						2						4						1	1			
	4			1			8	1		1										1							1			

tapping those from other series, and the calculations. In the second class fall those where the number of taps made falls short of, or is more than, the requisite number by one, and where mere uncertainty is indicated in the way the digit is tapped. The division where the digits are split into smaller ones is hard to place. It is possible that its errors fall in classes already given, and, if we could determine the facts in the case each time, the entire division might divide into either or both of the others. It at least tends to take on some of the characters of both classes, rather than a third form.

Possible origin of errors.—All classes of errors appear in the first group and all the subjects' records except 'Sd.' This first group cannot be assigned to fatigue effects, monotony, ennui, weariness or any of the causes that are found to operate at points in mental work where the subject is popularly supposed to be at a disadvantage in the amount and accuracy of the work turned out because of the previous work. We may conclude in tasks as difficult as those set here that the errors in the beginning of the series are caused by this severity of the task, the lack of practice and all that this lack of practice implies in the formation of more or less new sensory-motor coördinations, in the incompleteness of the memory process, and the inertia of beginning a new task of any kind. The subject does not possess the control over the process in the beginning that he later comes to have—it is highly conscious, as over against the fairly automatic character that it finally assumes. The fundamental motor coördinations and attitudes that shall enter into the process must be gathered from the tasks of the previous hour, or from a resting period, and brought under the dominance of this particular task. The new sensory-motor coördination must be related to these more fundamental and permanent motor relations.

The first part of the psychological task is to establish this preliminary control. How much of the energy of the body as measured in the number and intensity of strain conditions of the muscles, shall be used in the present task? This question must be answered independently of the immediate sensory-motor coördination of the recording part of the motor mechan-

ism. Upon this more deeply lying motor organization also rests the possibility of the retention of practice gains and the regularity of the process as a whole.

This does not deny the necessity of the 'external' motor process and its important rôle in practice and fatigue. Reserve adjustments of this second type may remain idle throughout the ordinary psychological experiment. In such cases the retention of practice is precarious, frequently impossible, and always ephemeral. It is only as the practice effects of the single test, increased rate and ease of working, become enmeshed in the relations existing within this deeper lying coördination that we reach the stage of firm practice retention and the level of skill. When the adjustment is momentarily satisfactory the task is not yet complete. There are continual trial and error processes going on in the effort to select some attitude that shall suit the situation still better and require a smaller expenditure of energy in accomplishing the task. This is in general the problem that must be met in the beginning of the series; hence the probability of all kinds of errors entering into this group is quite high. An organized coördination that will carry the task for a time is easily picked up in the majority of cases (as is shown by the rapid diminution of errors and the short period of comparatively few errors that soon appears) but is never lasting.

The errors that drop out first show this; they belong to the group that finds its origin in the more central processes, as they are usually called, and belong to what Sikorski called 'psychic errors.' Parts a, b, g, h, and j of Table V show the speedy diminution of errors of this sort, our class I. The other parts of the table, c, d, e, and f, continue to appear as errors in a single test somewhat longer and even alternate with these 'psychic errors' in several places. The main characteristic is, in the case of the mechanical errors, a tendency to spread along in the series as a more constant phenomenon than the others. This is not wholly true in individual cases. But, if we refer to their alternating character and their tendency to scatter along the series as accompanying phenomena, we will find it possible to distinguish in every case between the two classes

TABLE V.

MINUTES	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Class I.																				
Totals	25	16	11	6	5	9	15	13	14	22	17	16	11	10	23	15	19	17	15	11
A. V.	10.5	1.5	−3.5	−8.5	−9.5	−5.5	0.5	−1.5	−0.5	7.5	2.5	1.5	−3.5	−4.5	8.5	0.5	4.5	2.5	0.5	−3.5
Pos. or Neg.	+	+	−	−	−	−	+	−	−	+	+	+	−	−	+	+	+	+	+	−
Class II.																				
Totals	27	27	33	28	31	41	35	38	41	29	32	28	32	44	42	34	28	32	26	15
A. V.	−5	−5	1	−4	−1	9	3	6	9	−3	0	−4	0	12	10	2	−4	0	−6	−17
Pos. or Neg.	−	−	+	−	−	+	+	+	+	−	0	−	0	+	+	+	−	0	−	−

Table V. This table gives the totals of the two classes of errors, to show the difference in distribution. Besides the totals are given for convenience, the average variation and a column of + and − signs to indicate on which side the deviation is and how much this deviation amounts to in each minute of the tests. The totals do not exactly correspond to the whole number of errors as given in the preceding table, Table IV; errors that could not be placed at all were omitted. The detailed tables that were made out and of which this is an abstract show exactly the same phenomenon as the above, but more clearly since the individual variations in the distribution of the errors is not lost.

of errors. Class I (See Table V), distinctly tends to omit what we have called group two in the errors and begins toward the close of that group and runs over into group three with considerable strength. This can be shown by summing the errors for individual subjects by classes, when for subject 'A,' for example, after the first group is passed, there is almost a total lack of errors of class I, until the ninth minute, while in the minutes from five to eight this first class of errors does not appear at all.

Certain variations in the process were introduced in some of the experiments. These are shown by the horizontal lines in Table IIIa, and the other tables. Their nature was described on page 56. The effects of these changes were so slight that they need not be discussed at this point; we shall refer to them again in connection with a later series of experiments.

We may summarize the more important phenomena of the objective records as follows:

I. There is a decided fluctuation in *errors* and a fluctuation, somewhat less noticeable, in the *rate* of tapping in each experiment. These fluctuations are such as to give 'groups' of errors in any given experiment. These groups reach their maximum at variable periods, but, in general, occur about two or three minutes apart.

II. The errors when arranged in two classes with reference to the more or less mechanical character they exhibit, follow this grouping in a very peculiar way; to wit, those that are considered more mechanical in their nature, class II, appear in groups that almost alternate with the first class or 'psychic errors,' and always appear first. Groups two and three of the errors seem to show a quite definite relation in this respect, group two being predominantly of the second type of errors and group three being principally made up of the first class of errors.[1]

III. Group two of the errors as preliminary to group three apparently appears at the point where, if emotional excitement be great, or the subject be in a subnormal physical condition,

[1] This relationship between the error classes offers a possible explanation of Cattell's observation referred to above, p. 20.

the danger of breaking down is greatest. Four subjects failed to pass this point in two or more tests apiece.

IV. The greatest number of errors in the less complex series fall in what we have called the mechanical class. Apparently the more superficial coördinations are concerned in the early practice effects in simple tests and in short series, and are more easily interrupted under the stimulus of the light fatigue phenomena originating in deep-lying sources during the early part of a test.

V. Increase in errors and increase in rate are not always jointly appearing phenomena; on the contrary, there is a much greater possibility that with a steady increase in rate there will be a fairly constant production of errors of both types.

VI. Individual variations are noticeably consistent with the general disposition and the particular physical condition of the subjects.

e. Introspective Accounts.

The summary of facts as indicated by the objective records is very much clarified by the introspective reports that accompany the records of each day's work. These records of the introspections have been used throughout the previous discussion to explain individual anomalies in the records. It seemed better, in order to indicate the actual relation of the introspections to the facts as they appear from a purely objective standpoint and to emphasize the importance of the introspections, to put these records in a more systematic form than any simple reference to them in the previous discussion would allow.

Imagery.—In recalling as the tapping proceeds, those who use visual imagery report that they simply "wait for the image of the next digit." When it doesn't come, they are compelled to recall the one just tapped or the preceding group in order to start the new one. Curiously enough when the image is lost, there doesn't seem to be any immediate group of sensations that may be called on as is distinctly done in the case of motor imagery; but there is the feeling of waiting for the image. Another method pursued is to start the tapping process in

the hope that before too many taps have been made the digit will appear. Here the tapping clearly aids in the control over the imagery or the activities that condition this imagery.

Imagery alone does not prove sufficient to maintain a series in perfect order. The subjects report at different times the use of numerous and varied artificial means in continuing the process. These are used more frequently at points where the transition from one cycle to another is made. In actual 'breakdowns' resulting in errors, these artificial aids are conspicuous by their absence. Conscious control reduced to the minimum has literally failed in these instances to keep the requisite material on hand and the subject is left hopelessly lost and helpless regarding the next move. The field of consciousness at such moments is limited to the consciousness of being lost or of the last digit tapped. Upon the richness and closeness of relation to the previous process depends the possibility of proceeding without serious delay. The breaks caused by the material itself are, on the other hand, full of imagery and sensation processes, and it is only a question of proper correlation before the next step in the work appears.

Attention and Imagery Shifts.—In this connection two or three quotations may be made from the actual reports of the subjects. Subject 'Dy.' had, as we have mentioned, a distinct rhythm that was 'already in her finger' and she correlated this in the control of the series with the click of the lever and the heart beat. In this subject, we have therefore a definite method of control for the introspections. She reports that frequent errors were due to 'going off,' a light semi-hypnotic state apparently, and when consciousness returned she was lost. This return was accompanied in the earlier series by a distinct break in the rhythm, but later it frequently occurred without this rhythm check. In these later breaks the 'mind became a blank,' there was 'totally dispersed attention,' 'nothing would come.' When consciousness did return it frequently found that the finger tapping process was still going on, but the subject usually had little confidence in the correctness of the results. Subject 'A.' reports two types of consciousness connected with the errors, (*a*) simple mind wandering and (*b*)

cases where the calculations went wrong or the tapping wasn't kept track of until the correct number of taps had been made. Sometimes subject 'A.,' who used visual imagery, could not get the digit to move in from the peripheral imagery field to the clearer center; it remained vague, blurred, and uncertain. In tests where changes were introduced all subjects report distinct feelings of relief, though frequently this did not mean that the new process went on any better than the preceding one. This shift was occasioned from without. But all report similar voluntary and involuntary shifts of bodily position and attention that were distinctly conscious and produced exactly such relieved situations. Unconscious changes of a similar nature could be observed, as when changes in the amplitude of tapping appeared on the record that found no introspective basis.[1]

The determination of changes in the visual field is far more difficult and uncertain. In some cases it is impossible, since the field is usually shifted continually, *e. g.*, as in following a number form. An introspection condensed from three observations by one subject follows. It is a partial attempt to describe the very vivid experience of this subject, but no confident correlation is or can be offered with the objective records or with the errors of these particular tests.

The principal point noted was the fluctuation in the visual field of attention. This was not the field of regard since the eyes were shut, but during the progress of the experiment the apparatus in front of the observer was more or less held in the focus of consciousness by means of visual images. In this whole field there was also a consciousness of the retinal blurs and after-images that never wholly leave the eyes of the subject even in the densest darkness. Both of these fields would vary in size and content, during the course of the experiment, somewhat as follows. The attention presumably wandered and this field would gradually narrow till it became a mere dark or light spot in the idio-retinal field, then it would suddenly enlarge to include the most distant part of the apparatus or the peripheral portions of the field.

Accompanying this narrowing of the visual portion of the field there went

[1] We hope to show in certain control experiments that will follow that it is just such bodily changes and attention shifts that in a large number of instances prevent the subject from breaking down utterly under the complete unity of the process. 'Monotony' is perhaps the more common term for this situation, when described in terms of 'interest.'

a feeling analogous to that which precedes a fainting spell. Or it is similar to those conditions that appear when one has over-exerted one's self and a vague, immobile condition of consciousness is approached. There is also a feeling that this is very near the sleep stage of attention; that if something, one doesn't exactly know what in the beginning, would only give way, sleep of the most delicious kind would come. To offset this pleasant situation and, as nearly as the observer can tell, exactly within the state we are describing, there is another factor that prevents the consummation of sleep. This latter is a tense condition, somewhere outside the field immediately in the focus of attention. It is very difficult of location. It may be merely the marginal awareness of the digits as they are said in the course of the tapping, or the actual awareness of the narrowing field (though this last is certainly very indefinite since it is rarely detected until the condition described has passed away). It may be any one of the groups of 'strain' sensations that can at any moment be brought into the general consciousness. This tenseness holds on, however, and is the one thing that seems to keep consciousness from dropping away entirely.

During such a moment of inactivity, the difficulty of continuing the tapping seems very great. Only the digit that is being tapped at the moment is known. The subject feels at the mercy of the associative process and if it does not turn up the right digit at the close of the tapping nothing can be done about it. So small does consciousness sometimes become, that even the whole digit being tapped is not known. There is present merely a felt condition that something is being tapped, that there is a possibility of stopping when the time comes, or of the other digit coming in at the right moment and stopping the present process.

The 'strain' groups that have been noticed are: tenseness in the muscles of the ears; a group of 'strains' at the back of the neck, aggravated by the position of the subject; some that come from the muscles of the legs and feet where there are any places of complete stillness; from the trunk muscles; from around the eyes, the forehead, and from the back portions of the scalp as well. Not all of these appeared in the same narrowing period, nor was it noticed any time that more than one appeared. The point of origin varied with the position in which the subject was sitting, or the position of the head; the present vigorous condition of any portion of the body determining, apparently, in a very noticeable degree, the place that should first produce the 'sensations' thus noted.

In passing from this state practically the opposite set of conditions obtain. The field enlarges; the strain sensations drop out of notice; the apparatus in front is again visualized or the whole idio-retinal field returns; the digit being tapped, its position on the series, some notion of the place in the whole group

that is being tapped, and of the time that the subject has been working, return to the observer. In such instances it frequently appears that some irrelevant thought has led the subject away. Once started the whole process seems to disappear and leave consciousness, as described.

Such introspections find corroboration in the experience of other subjects. Subject 'Wl.' found that the narrowing of the field occurred several times in the course of a simple automatic series and less often in the more complex series. Subject 'Pn.' reports a very interesting condition that finds its corroboration in the error 'grouping.' This is a temporary vagueness of imagery and it extends over two and three successive digits. He finds that an error will appear, due to some break in the coördinations, and before the thought of this conscious shift can be removed several other errors have appeared.

Sensations.—The introspective accounts are rich in material, but what can be given at this time must bear on 'tension' and 'fatigue sensations' alone.[1]

Source of fatigue.—After the first trials with the movement, the almost universal practice of the subjects is to cease reporting any fatigue of the lip, the one part of the motor mechanism that was in constant motion for the recording process. Cases in which fatigue was reported were found to be due to the position of the apparatus, to the reduction of the cutaneous

[1] In this connection, is appended an introspection made by the writer in one of his first tests with the apparatus when there had been practically no habituation for the apparatus.

"During the experiment several groups of muscles came into prominence that had not been in action at the beginning of the test and were not used in the test when only an average attention was used. Noticeable were the tension of the forearm as it rested on the table, the tension in the forehead, the increased tension of the back of the neck, and extending up to the back part of the head farther than usual, an increase of tension in the lips, the eyes, and about the head generally. The lips did not appear in the least tired, i. e., report the fatigue sensations, which were in every case apparently reported as from the points of origin mentioned above. The lip movements did in some instances show a tendency to slow up. The attention wandered in several instances, and the errors seemed to bunch themselves more than was noticeable in the former tests. This variation in attention was corrected by turning the attention not to the next number to be tapped but to the lip movement, then to the digit. It seemed to be necessary to get control of the lip movement in a firmer way before anything could be done with the mental process."

sensibility by too much pressure, or by the rapid movement of the high speed tests. Subjects 'A.' and 'Ch.' both reported the special place of fatigue in the region of the eyes, forehead, temples, and scalp. Subject 'Ch.' also reported noticeable strains from the diaphragm muscles. All the subjects reported the fatiguing sensations of the continuous posture. The back of the neck was a frequent spot for the location of these sensations, probably due to the fact that the subjects felt that they must retain the same position with respect to the apparatus. Subject 'Rn.' found that the sensations appeared in any region of the body and that they seemed to have no local preferences; such was also the case with subject 'Ym.' 'Sd.' reported a desire to stretch and a general tendency to move at the close of the experiment. 'A.' reported greater fatigue in the lip than was reported by the other subjects. The major portion of this sensation was, however, a cutaneous one occasioned by the pressure of the apparatus against the lip. The separation of the two sensations, cutaneous and muscular, came out very distinctly for this subject in another experiment made to determine the relation of finger tapping to lip tapping. Here the obvious control in the first stages of the test was the cutaneous factor. Soon it became impossible to carry on the process with any degree of certainty and attention was found to move to a control that had apparently not been used up to this time as a separate conscious control, i. e., the kinæsthetic factors. These were used for the remainder of the test. This experience was reported by others in the lip movement and when questioned, none of the subjects has been able to locate in the actual muscles used the fatigue that came temporarily. This 'fatigue', or numbness was always located in the cutaneous region pressed upon by the apparatus and not in the deep-lying tissues.

Tendency to Sleep.—Four of the subjects reported an overwhelming desire to go to sleep during the course of the experiment. This sleepiness had to be fought off continually after the first control over the material had been obtained. Apparently it constitutes a hypnoidal phenomenon similar to the experiences reported above wherein the field of consciousness was reduced noticeably during the course of the test. In such experiments, we have a single process offer-

ing little in the way of variety, yet requiring a steady strain of attention during its course. There are no extraneous conditions except those that are as continuous as the process itself, the stillness of the room, the whirr of the motor; a typical hypnotic situation. The result is that the sleepiness induced has little or no imagery in it. When sleep appears, the dream imagery is frequently the imagery that has been pushed out at the beginning of the experiment. In one instance this amounted to a dream of some proportions. The subject found herself just ready to embark on a steamer for Europe. When almost ready to step aboard the steamer, a question was asked her in the dream and at the moment of the decision regarding it, she awoke to the tapping once more. Typically opposite results were obtained from three other subjects.

Several times just before the experiments, each of the three subjects, as they recalled the course of the previous tests, remarked that they felt as if they would go to sleep this time. But in no case was any desire to sleep manifested during the experiments, though mind wandering and mental lapses were reported frequently. These subjects actually reported in all these cases that they were much wider awake at the end of the experiment than at its beginning; sometimes highly excited at the close and eager to go on or do something else at once.

In connection with these varying reports, we may mention a characteristic change in the breathing curve. Those subjects who reported the tendency to sleep exhibit, in experiments where a breathing curve was taken, a very slight but definite slowing of the rate of breathing. In subject 'Sd,' this amounted several times to a decrease of two respirations per minute during the course of a twenty-minute experiment. Subject 'Dy,' who manifested no tendency to sleep during the experiment, presented considerable irregularity in the breathing curve. Only once did she show any tendency to breathe more slowly during the course of the test. This one was the test in which there was no danger of exhaustion and a decided decrease in the rate of tapping. For one or two of the subjects, the breathing curve shows at fairly regular intervals a deep inspiration. Only some thirty of these breathing curves were taken, so the results cannot be more than provisionally related to other characteristics of the work curve.

SUMMARY.

It is hoped that enough of the large amount of material gathered in the course of the experiments has been presented to indicate the value of introspection, uncertain as it is in such continuous series. Among the details of the introspections presented, special mention may be made of the following:

I. 'Sensations of fatigue' arise suddenly, that is, they appear in consciousness and take the center of the field at times,

where they function as ordinary sensations. At other times exactly the same situation produces, not an attention process centered on the 'fatigue sensations,' but one focused on some other part of the process, e. g., something in the field of vision, in the other imagery of the process, a movement that appears afterward in consciousness, or on any process whatever except the immediate image that would have continued the work in hand.

In these situations, we would suggest that the sensory processes have been instituted as before, but the coördinations are established, and instead of producing a demand for conscious control, the stimuli have resulted merely in a modification of the process on the basis of the immediate habits used in the present situation. The length of time that these sensations function as conscious processes depends probably on the working habits of the individual, the relative difficulty of the present situation, and upon the length of time that the subject expects to continue the task.

II. When the introspections are made at the close of a fifteen or twenty minute experiment, errors are reported as due to breaks in the rhythm of the process (*a*) from unknown causes, (*b*) from sources within the imagery or mental work of calculating, (*c*) interference of outside stimuli, (*d*) appearance of ideational processes from unknown sources, and (*e*) *sensations which when localized interfere definitely and when only vaguely present tend to slight degrees of confusion.*

III. *Introspection* reports the greatest difficulty as arising from two sources, first the monotony of the process, i. e., the loss of the excitment and the tendency to lose the stimulating 'strains.' In the second place is to be mentioned the difficulty of maintaining the tension (attention ?) out of which the particular mental process is issuing. This tension, if lost, means that the conscious process will also be lost. *The maintenance of this situation seldom involves a steady, specific strain.*

Discussion. In the progress of a particular mental activity, there appears from the above to be for each individual a core of closely related sensations and images, based partly on the ideational types that are commonly recognized as motor, verbal,

auditory, visual, etc. As a test of this hypothesis let us try to describe what is to be expected in any single case of incipient fatigue. If our particular individual selected for the test be of the hand-motor sort, then we may expect such things as writer's cramp, the feeling of limpness in the hand and the wrist, or the rise of that more indescribable thing called an indisposition to work, lassitude. The proper coördinations that have been furnished the 'vis a tergo' are receiving less and less of the stimulating sensations that constitute the 'tendency' to mental activity.

In these experiments, the control of the process does not seem to reside in the lip movement, but rather in the tensions that constitute the basis of the motor process. This control is of a sub-conscious character and not in the focus of consciousness. When the tensions do come into the focus, it is by means of a distinctly new set of sensations, 'fatigue sensations.' The entrance of these sensations is the 'cue' in every instance noticed for a change of attention either to the point of origin of the sensations themselves, or to some more interesting topic of thought.

The recovery from wandering attention or from any difficulty of recording is not always first through a mental image, but frequently through some reference to the particular muscle group guiding the record. Any deep-seated disturbance of the sensations ('stimulating factors') arising from the particular points under tension indicates that the focus of attention has varied. Tension rather than movement is the immediate controlling factor. Though movement may constitute that portion of the process that is determining the locus of the tension, the tensions themselves may frequently be elsewhere than at the places of overt movement.

The location of the controlling sensations is not usually noted in any disturbing sense until the sensations that arise from that portion of the process are actual 'fatigue sensations.'

The response to this is a relaxation of the point of tension and a tendency to movement in the parts affected. In the muscles actually moving in such experiments as these the fatigue sensations are practically *nil*, and are not the ones that make us

say we are 'tired.' These parts are best irrigated and are thus kept in condition for their work. The other parts are in less satisfactory condition. This seems to point to the theory that fatigue is due to the excitation of special nerves by the accumulation of one or more of the three fatigue substances beyond a normal amount. This accumulation appears first in the tense non-moving muscle. The accumulation finally takes place in the muscles that are working but always a considerable time after the fatigue sensations elsewhere have superseded the sensations of tension and strain. There was no experiment at this point to test the possibility of transference of the control process, but if this description be true, it seems to indicate the possibility of a way out of the attention-wandering experience entirely. If the organization of the muscle controls be closely enough connected and arranged in serial order through habit, etc., there will be no need of any attention-wandering at all. For example, on the basis of habit the effort might be transferred from chest muscles to abdominal muscles or the tension be even moved to muscles of the extremities in the course of a single piece of mental work. This transition can be made on the basis of habits previously established, and while the mental process is going on, the transference of control from one fatigued set of muscles to another could take place without disturbing the continuity of the specific mental process.

f. Control Experiments.

1. Using a rapid rate of tapping.—Certain control tests were made to test the validity of this view of mental fatigue. We have at various times mentioned a group of experiments where the rate and conditions were made comparable to others' tests of this general nature. This high rate is, of course, impossible of direct application to the more difficult calculation instituted in some of the series. Instead of asking for a high rate a combination was sought that gave the most rapid rate possible and still kept the errors within limits less than one error per digit of the cycle. This enabled the observer to feel that the work was going well and that special confusion on account of the speed did not enter.

TABLE VI. Errors.

SUBJECTS	Ym.				Ch.			Rn.		A.
SERIES	a	b	c	d	b	c	d	a	b	a
1st min.	4	4	2	3	1	0	0	3	8	5
2d "	0	1	1	0	3	0	4	3	1	5
3d "	4	5	2	7	3	1	1	4	5	3
4th "	5	4	1	2	1	1	2	16	8	1
5th "	1	1	2	4	2	1	4	8	18	0
6th "	3	3	1	3	1	3	3	5	14	4
7th "	4	4	2	2	4	0	3	8	7	2
8th "	?	2	5	4	6	2	1	5	7	5
9th "	4	2	1	5	3	3	5	6 ?	?	5
10th "	2	2	4	2	6	1	3	7 ?		1
11th "	5	4	5	4	4	2	0	15		3
12th "	6	2	2	3	10	0	5	8		4
13th "	7	3	1	2	1	1	3	11		7
14th "	4	6	5	9	7	1	4	13		1
15th "	4	5	6	3	5	1	5			3
16th "	9	3	6	5	3		4			7
17th "	10	2	1	0			1			
18th "	2	2	3	5			3			
19th "	2			5			5			
20th "				6			3			

Table VI. Selected from results of 'rapid series' tests. The rates, with the exception of speed, are so nearly comparable to the preceding tables that they are not given. 'Series' in the table indicates the order of tests as given on successive days.

Table VI shows the results of the 'rapid rate' experiments. The initial group of errors is lengthened and group two pushed up on this account to a position near the third group which occupies its usual position. Otherwise, the errors appear in their ordinary manner, slightly increased in amount in subjects 'Rn.' and 'A.' 'A.' is particularly noticeable in this regard, since his records in the earlier tests are quite free from errors. The very great proportional increase in the rate indicates the main reason for this change in the errors. We stated that the distribution of errors within any single experiment does not depend in any appreciable degree upon the rate used in the experiment. These tests suggest that the absolute rate of an experiment determines in large measure the total number

of errors that the whole experiment will contain. This tendency is modified, naturally, by the proportionate difficulty of any cycle of digits and its accompanying calculations. We may repeat this as follows: *The absolute number of errors in an experiment depends largely on the rate and difficulty of the experiment, while the distribution of errors bears slight but positive relation to the changes in rate that occur within a given experiment.*

Introspectively, such attempts at speeding indicate the futility of attempting to get at an absolute rate of the mental process through such motor processes as these; i. e., isolated motor accompaniments that are correlated with the mental processes simply for the purposes of such recording. The motor process of recording can in no wise equal the speed of the complex motor adjustments (fusions) that are in charge of the imagery in adult experience. The mental processes wait throughout on the recording apparatus and the approach to them is not through the speed of isolated movements in any one portion of the body, but through the coördinated activities of the entire organism with reference to this momentarily isolated portion. Thus shifts in the recording movements affect the coördination not merely by relieving the particular part that happens to be active, but also by producing a rearrangement of strains in the total coördination.

Effect when finger is used to tap the digits.—The change from the lip-tapping to the finger-tapping, where the subject had used the former through the greater part of the year's experiments, shows no extreme variation on this or other points. The selection of the lip as the ordinary method for the experiments enabled us to make this comparison between the establishment of a new coördination and one that was highly automatic. Possible differences appear. The rate is more rapid. Certain rates already exist in tne hand mechanism that cannot be modified on the spur of the moment and must be used to carry out the mental series. This rate represents quite fairly a regular speed for the coördinations of the hand. Differences between the right and left hand indicate a modification of this proposition that is quite interesting. *When the right hand, lip, and left hand, were used in alternation to record the cycles within*

a single continuous mental series, the left hand showed the highest rate throughout the earlier tests. Only subjects that were right-handed were used on this experiment. But the result seems to indicate that we are dealing with a relationship between two separable conditions. Certain rates are worked out in the hand mechanism, the right hand in these cases is primarily used to establish this rate and is thereby more closely and definitely related to its appropriate coördinations and 'strains' in the deeper proprio-ceptive mechanism. The left hand follows by the transference of practice and irregular trials of its own, attaining a speed approximately equal to that of the right, but lacking the intimate and more thoroughly established connection with the deeper coördinations. Two things follow from this situation when a new mental process demands expression. The left hand starts off at the rate 'artificially' imposed upon it by the right hand from previous practice. As opposed to this the right hand because of its closer relationship to deep-lying coördinations indicates at once in its slower rate the difficulties in the new mental process. In the second place consciousness reports this condition of the left hand in the awkwardness of the tapping and the more rapid rise of 'fatigue sensations' and fatigue itself.

Highly complicated series.—In a group of twelve experiments especially arranged and ten others arranged for another purpose, the calculations and complications of the mental process were raised to the limit of the relation between the retentive powers of the subjects and their ability to calculate during the recording process. The rates showed nothing beyond a slightly higher practice change and in one subject, 'Ch.', where the work was quite difficult, much higher fatigue effects. The. errors follow their characteristic changes with two exceptions Group one tends to continue longer than usual, a point noticed in the 'rapid' series above, and there is a slight masking of the later groups in the records of 'Ch.' because of emotional states brought on by the confusion attending the difficulty of the series.[1]

[1] One striking instance out of the many, for the necessity of careful checking on these introspections may be mentioned. A cycle of ten digits, 3, 2, 2,

New directions during the test.—To test the continuous complications of a series, a group of experiments was carried out in which the subject received the directions at variable periods through the course of the experiment. The subject would start on a series of digits memorized beforehand and at intervals selected by the experimenter would change the calculations on the memorized series of digits. Ten and twelve different directions of the nature of the calculations described in the experiments of group II were given in a single test of fifteen minutes in length. Care was taken in the series to make some of them as homogeneous as possible. Others were started by an easy direction or two and ended by the same direction, or ones similar in their degree of difficulty, in order to test the possibility of general fatigue and the effect of the intervening difficult calculation on the automatic processes.

Such tests obscure the results as found in the other experiments by increasing the total of the errors throughout the test to a point that practically equals the errors of the first part of all the other tests. The tendency to disturbance in coördinations and 'strain' groups is far less noticeable. In fact, it is practically unnoticed because of the demand on the attention and the interest attendant upon the frequent introduction of a new form of calculation. This undoubtedly takes the place of the shifts and error groups that are familiar in the continuous homogeneous series of the other experiments.

Alternate tapping.—The second question, raised in connection with fatigue sensations and the 'strain' sensations, was regarding the possibility of reducing the error fluctuation by

4, 3, 2, 4, 4, 2, 3, was given and memorized. The tapping was begun and continued for thirty seconds when the subject stopped and reported as follows: her mind had become a blank and no imagery could be found (after the final correct taps about ten seconds were spent in an effort to recover, according to the record). In repeating the cycle as the double fours were noticed, she thought that these would be difficult to recall in their proper positions and while thinking of this danger finished the series. Here *according to the introspection* the mind became a blank and no more images appeared. The record shows that the cycle was tapped once correctly and up to the second four of the pair the second time with no errors. The thought of the specific difficulty had erased all memory of the process that had continued during the persistence of the idea.

changing the muscle groups used in the recording process itself. Schuyler and Swift, in their paper, indicate the decidedly positive character of typewriting as far as the reduction of localized fatigue sensations is concerned. This observation is one that is made by many users of the typewriter. The localization of these sensations is much more difficult than when pen or pencil is used. An analogous observation is presented in the experiments of Sherrington where the muscle response is elicited for a considerable length of time after most observers have reported fatigue, simply by moving the place of stimulation. Exactly the same sort of issue regarding so-called intellectual or mental fatigue has been experimentally brought out by the work of Lombard and Hall with the ergograph. As a theoretical result, the fluctuations in errors and mind wandering may perhaps both be largely eliminated by the arrangement of a series of tests that alternate the processes used in tapping the records. Thus the habitual working rhythm of the various subjects could be eliminated so far as concerns an inferior quality of work appearing at certain periods. A tentative test of this was made on several of the subjects. It involves, however, for complete control a definite knowledge of the physiological rhythms of the individual and also of the habitual methods of working, so that nothing more than a preliminary illustration of what is meant can be offered at present.

The preliminary apparatus for this test is as follows: the lip arrangement for recording was used, and in addition the tambour attachment that was used in the finger tapping was arranged for both the right and the left hand. These three tambours were attached to the same recording tambour and the record run as a continuous one on the drum. (This control test is the one mentioned above in the preliminary statements regarding apparatus.) Highly complex memory series were used in these tentative tests. Hence the entire significance of the change in recording is not shown. Notwithstanding all the difficulties indicated, the records that were taken show a decided disturbance of the usual rhythm of fluctuations and show plainly that we are not dealing with an isolated mental process when we speak of the quality of work and of fluctua-

tions in intellectual work. (It is in these experiments that the records of the left hand show, in the two or three earlier tests, a rate that is regularly higher than that of the right hand.) The introspective account of the changed method is a decided one. All five of the subjects who tried it were clear in the conviction that had they been using this method throughout they could have eliminated many errors and have been much fresher and more interested in the course of the experiment. Subject 'Ch.' still reported slight fatigue about the eyes. The difficulty of the experiments in her case and the necessity of using the number form in order to remember the series materially modify the conditions of her record. The method of recording is not calculated to modify this condition to any extent. We feel certain, notwithstanding, that no one would think of looking for error groups or regular fluctuations in this series of records.

SUMMARY.

The actual mental condition of an individual at any time is a function not chiefly of the particular set of muscles in operation, but of the ability of the person to maintain the more fundamental strains of the entire body. Of course, whenever the method of recording be such that the muscles recording the work done must enter into the qualitative changes that appear on the record, then the results will be modified materially by such conditions; for example, if the muscle gets excessively tired, this abnormal condition of the muscle will affect the records of the mental work being done. *These error groups are, however, in our view an argument against the possibility of considering mental work as anything apart from specific, coördinated muscular response.* These coördinations are, moreover, not to be confused with particular muscular responses. The mere recording should not materially affect the real results of the 'mental' work. It is to be noted that this must be said with a reservation respecting secondary effects that the working of any group of muscular responses may have on the

'strain' conditions that already exist.[1] The effect that the transition of the actual and specific activity from one recording muscle group to another will have on the physiological conditions that are maintaining the heightened cerebral activity, should also be noted.

It is not denied that the particular *deep-lying* group used in recording is affected. Both as a working muscle and as a part of the neuro-muscular process, it will exhibit all the phenomena of mechanical stimulation of muscle and will also mediate sensations that are real fatigue sensations, affecting in both ways the smoothness of the cerebral activity.

Here we are dealing with a more general phenomenon, one that is not conditioned merely by the possibility of the circulation diffusing fatigue substances, generated in the working parts, to the more remote parts of the muscular system. Diffusion of fatigue substances cannot be made to explain the fluctuating phenomena of the error curve nor the introspective report of those subjects who report definite recoveries in their ability to do the work in hand. These recoveries are independent of any possibility of changes in the circulation as such and of the distribution of a chemical substance throughout the body. In other words, these sensations arise suddenly while the production of fatiguing substances must be considered as going on at a more regular rate, or, at least, diffusing more regularly.

A shifting of the 'strains,' that in themselves are productive of fatigue substances, in cases of high mental tension will account for the automatic shifting of the specific seat of the strains and the production of a new center for the 'vis a tergo' sensations. In the majority of cases this transition is not accomplished unconsciously and we have the appearance of the fatigue sensations and the habitual attitude of the majority of workers with respect to these sensations; the loss of interest, the shifting of attention, the feeling that one must do some-

[1] The numerous and interesting effects of different incentives upon the amount and nature of fatigue have not been made a subject of investigation here. They are, however, relevant to the main problem, i. e., the relation of sensations of fatigue and amount and character of work done. Wright's article (108) and Bücher's (17) indicate fruitful lines of inquiry on this particular point.

thing else since this new mental condition is considered indicative of a need for rest. As a matter of fact all that it does indicate ordinarily is that *a center of kinæsthetic activity is calling for readjustment.*

IV. GENERAL RESULTS AND CONCLUSIONS.

Before passing to points of general significance that the data and form of statement of this paper have suggested, reference will be made to the specific points that were offered as problems most insistently rising out of the previous investigations.

Physiological conditions.—The experimental work outlined here suggests what seems to be a very fruitful field in the arrangement of experiments in continuous activity. This is the effort to get conditions that shall approximate the conditions that obtain when the subject is working under the impetus of normal incentives. Naturally, such an ideal means the introduction into the experiment of somewhat of the complexity of the study table, and more of the atmosphere of the investigator into the attitude of the subject. It seems to the writer at least that on psychological grounds, an investigation ought to lead somewhere for the subject as well as for him who plans the test.

The plan of the experiments here carried out has tended to produce in the minds of those who have watched them as they have grown, the conviction that it is only by such 'at will' methods that the correlation of the physiological and psychological facts is going to be brought about. For example, *the work of this paper suggests still more strongly what lies in the background of most psychological investigation, that the rhythm of certain highly automatic processes modifies materially the character of a short and circumscribed experiment.* Had the writer developed more fully certain other material that seems to lie beneath the more obvious data that have been offered, he would argue specifically for the correlation of qualitative changes with vasomotor rhythms and similar physiological processes. For example, it seems quite clear in many cases, that the errors while grouping themselves in these larger 'muscle' groups also tend

to scatter single errors along the course of the work in arrangements that suggest these more fundamental physiological rhythms.[1] At present such statements must be left for further investigation and more thorough corroboration.

Moreover, such problems on the road to solution will enable the psychologist actually to make an adequate analysis of the complexes he so trustingly terms kinæsthetic sensations, organic feelings, and more recently 'attitudes.'

Furthermore, such a method presents the possibility of a less forced attainment of the ordinary conditions that are set in the laboratory. Several of our subjects present the spectacle of attaining the maximum of attention and interest without the insistence of this through express directions from the experimenter. We are thus offered a chance of investigating the actual 'warming up' process and the varying time in which it is attained by the same subject on different occasions and by different subjects under approximately similar conditions.

Fatigue and practice.—The relation between fatigue and practice ought to receive further illumination from such methods as are advocated here. In the secondary emphasis that it has received in this paper the conviction that they are not related in precisely the close correlative fashion that certain investigators have thought them to be, has become more firmly fixed. Fatigue as shown by our work, by Thorndike, by Ebbinghaus and others, is, first, a very immediate function of the mental process, and, secondly, a reduction in the general capability to maintain the production of a maximum amount of work. We have stated above our own conclusion as the curves of our experiments seem to indicate them.

We may offer here as a result of this point of view, the fact that immediate fatiguability is frequently independent of practice power and practice gain (in this statement we do not go

[1]The work of Bonser (13) certainly offers very striking corroboration from the physiological side of the presence of these longer two and three minute fluctuations in the mental work, the discussion of which constitutes a large part of the experimental portion of this paper. Space will not permit a discussion of the numerous articles on daily rhythm, individual working hours, etc. These have been quite carefully summarized in a paper by Marsh (60).

beyond the suggestions of Wimms or Schuyler); that the possibility of practice and the retention of practice does in a very real sense depend upon the general fatiguability of the organism. By fatiguability of the first sort we mean in this statement that which shows itself in definite fluctuations in quality and quantity of work during continuous work. This is not the fatigue which Kraepelin argues is related to the practice curve. General fatigue according to his definition and ours actually diminishes the power to work for long periods, and is correlated with practice.

Without the 'fluctuating' type of fatigue in mental or muscular work the possibility of any extended effects of practice ever getting into the deeper co-ordinations or 'strain' groups of muscles is very slight. We meet an analogous argument in Sherrington's discussion of 'negative induction.' (See pp. 9–10.) In the properties of the synapse he finds resident the possibility for the large variety of sensory stimuli to which the organism is open. If 'stimulus and response' from a single center were not subject to a diminution in activity by means of that activity itself, there might be, for example, an eye of far greater penetration than at present exists, but such specialization would mean the absence of, or highly diminished power of, other sense factors. Thus the fluctuations in mental activity conserve the power generated by the specialized process by shifting that activity from time to time in such a manner that the effects of the activity are gradually diffused to a more far-reaching coordination than is in immediate use. This transference may, of course, take place in both the positive and negative planes of transference of practice. As a result of either transference we have the so-called habits of work generated, and in the majority of adult individuals the distinctly apparent reduction of early plasticity and capability to take on practice gains (i. e., negative transference).

We should therefore agree in the main with the statement made by Seashore that fatigue is not general but specific in its nature. The type of fatigue that has been most in evidence here is capable of definite localization and description and belongs to the realm of psychological investigation. We do

not contend that the work curve is not dealing with real facts. It seems, however, that shifting the controlling mechanisms tends actually to increase the metabolisms throughout the neuro-musculature of the body. The generality of the fatigue symptoms that are dependent on decrease of protoplasmic structure in the tissues, is in some degree proportional to the susceptibility of the muscles to shifts in the coördinations, or to immediate fatigue. These facts are in their nature opposed to a general decreased power to work.

Quality of the work.—The number of these changes and error fluctuations, together with the rate of fluctuations, indicates the general habituation and endurance of the individual. This will be true on the same grounds that Kraepelin argues for the relation between the 'equilibrium pause'[1] (Gleichgewichts-pause), and the 'most favorable pause' as an indication of the high or low fatiguability of the individual, i. e., in terms used here, the greater or less susceptibility to coördination shifts. The quality of the work ought therefore to constitute a very important criterion for the evaluation and actual location of the factors involved in an objective work curve.

Consciousness and fatigue.—The complex of psychic accompaniments of fatigue is so varied and so common an experience that the attempt to analyze it into simpler groups has always seemed hopeless. We are confronted with 'feelings of effort,' 'feelings of well-being,' 'exhaustion feelings,' 'strain feelings,' 'sensations of fatigue,' 'lassitude,' 'limp' sensations, 'laziness' and a score or more that indicate the bodily attitudes from time to time. Out of the individual parts of the complex, formed by it, and determined by their proportionate relations, the 'feelings of effort' seem to stand as the final psychical representative. Beneath and just within the consciousness of effort we usually locate the feelings of strain; in serial order,

[1] By 'equilibrium pause,' Kraepelin means, as was previously stated, that pause where the amount of work done after the pause in a unit of time is equal to the amount done just preceding the pause. To determine the locus of such a pause, certain factors, as the loss of 'incitation,' decrease in fatigue, the introduction of an Antrieb at the recommencement of the work, etc., must be taken into account, hence the use of the technical term 'weighting' in the descriptive phrase.

they appear after the whole complex has had its turn in consciousness. Outside of these, appearing later still, and usually viewed as disturbing factors, we find the 'sensations of fatigue' placed. These are popularly supposed to be correlated in a negative fashion with what we have included in the descriptive term 'feelings of well-being.' It is not our purpose to maintain on the basis of any cursory analysis of this complex that these are all to be separated into clearly differentiable sensations. As a matter of fact one or two of them seem to belong to categories similar to those of space and time perceptions.[1] Outside this debatable ground, however, there seems to be sufficient evidence to attempt the isolation and description of the fatigue sensations. In the analysis offered certain distinct types of objective fatigue also appear with some distinctness.

Types of fatigue.—1. The first group of shifts in coördinations is that one which gives us the varied imagery of our most rapid conscious processes and is undoubtedly conditioned by precisely the same physiological situations as is the phenomenon designated by Sherrington as 'negative induction.' The varying length of these changes (from the immeasurably short shifts and 'blockings' of the more finely adjusted musculature that report themselves in consciousness as wholes of pleasantness and unpleasantness, as confidence in the possibility of doing the thing in mind, etc., on to the grossest muscular contractions that finally cease to respond to the effort of will) can be explained by the gradual refinement of the sensory end-organs lying within the muscles and their gradual coördination through long periods of use and practice with the neural pathways and synapatic mechanisms.[2] There is no apparent reason why

[1] See articles by R. Haynes in *Jour. for Philos. Psychol. and Sc. Meth.*, 1907, v. 4, pp. 601–606, on Attention, Fatigue and the Concept of Infinity.

[2] From this point of view, it seems almost impossible to place the actually controlling locus of the nervous 'blocks' in such evanescent functional units as the synapses. It is undoubtedly true that in them may be located the immediate cause of the shift to some other neurone system, associative or projective. The point, however, where it is first determined that this neurone pathway has acted to its capacity in this particular place and at this particular time, and the point where it is determined what neurone system shall follow, is not originally in the synapses but in the muscles themselves. It must not be forgotten that in so describing the situation we are aware that in the majority of instances

these changes may not go on indefinitely, following their own internal lead, as in reverie and mind-wandering. There is also no immediate psychological reason whereby to explain changes such as are usually found in interest and monotony and so on, beyond this 'fatigue' shift. As convenient modes of description, interest, monotony, etc., find their explanation in *the establishment of habits of thinking and of acting*, rather than in anything that is inherent in the objects themselves. The biological reason for shifts is well stated in the 'negative induction' theory of Sherrington.

Consciously, we say that our attention could no longer be held by the object in question, (we even attempt to measure the length of time one thing can be focussed by the attention) and we describe the arrival of new objects of attention and of new thoughts as due partly to the loss of interest in the old, to the lost fascination that any particular object may have had for us. These descriptive phrases seem explained on the basis of this habitual factor that has ingrained itself in the muscles and nerves of our organism. This neuro-muscular mechanism has come to offer the shifts at certain times and unconsciously we accept them and look at the object from a 'new' standpoint. At other times we consciously accept them as the 'gospel' and say that we must do something else or think of something different, retire, rest, or take recreation, must do anything in fact but modify the situation by refusing to be dominated by one single set of responses and, if this set fail to work, call upon another set or adjustment that will. A coördinated habit of mind wandering is the result; incomplete concentration in any task becomes the unvarying habit.

2. The above described process is more or less automatic; our mind wanders and we find it out afterward; we are not conscious that this has taken place till we find the mental process engaged on something that reports itself as different from that

this coördination is not a conscious one, but has dropped into the realm of automatic activities. In others instead of being manipulated directly by a conscious process the direction of change has become an habitual mode of response. In still others, a possible majority of the instances, the change is manipulated from the standpoint of the exteroceptive system, sense-organ adaptation and fatigue.

which started us off. But there is a more definitely conscious shift than this *post hoc* sort of consciousness. It is one that offers confirmation to the above statements. Within an habitual mental and muscle group tasks can be arranged to perform which will require the expenditure of more energy than is ordinarily used in the automatic process of shifting the coördinations. In other words, our habits of thinking are not usually 'set' for the longer periods of time. As Professor James (46) puts it, we are ordinarily working far below the level of our ability so far as the consumption of stored energy is concerned. This is, nevertheless, the condition that has led to our habits of shifting and forms the logical basis for the constant shift of attention that we find generally in the animal kingdom.[1]

In human life we can and do get conditions that demand the expenditure of continuous streams of energy. As a result, we have a more or less homogeneous piece of mental work continuing over long periods of time, that cannot be explained on the basis of a single adjustment of a single set of muscles or a single muscle. A second and higher habit is formed, the coördination of kinæsthetic shifts. If it were physiologically possible and biologically and methodologically valuable, as individuals we should much prefer to have the control carried on by a single group of muscles till the mental work had been completed. At this point, however, the chemist shows us substances produced in the working muscle, strain or active contraction alike, that are inimical to its continuous activity. The biologist tells us that the specialist, as a mere individual, must fail in the great life functions. The scientists generally demand not only habits but the power to break those habits. Each and all abjure over-specialization.

On the conscious side, if we push the muscle beyond a certain stage of activity, we are awakened to the fact by the appearance of a new phenomenon, 'fatigue sensations.' These

[1] Its biological variations are interesting. The cat sitting patiently at a hole awaiting the appearance of its mouse, the sudden stillness of the wild animal at the appearance of a stranger in its native haunts, indicate instinctive modifications of this internal organization.

are from this general viewpoint the correlates, on the psychological side of the process, of the 'negative induction' and automatic shiftings that we have described above. Two things are effected by the katabolism of the muscle under tension, the first is the functional shift from one synapse to another, the second is conscious and it constitutes the warning of the changes in the situation. Here is the point at which the difficulties of our analysis become apparent.

Fatigue sensations are of a certain definite kind but they are so intermingled with other sensations, of effort, of pain, discomfort and the like, that they are scarcely isolable in ordinary situations. If we take the experiments on the ergograph, we shall find three distinct groups of conscious factors present. (1) The 'feelings of effort,' (2) the pains of torn tissues and muscle fibers, and cutaneous pressure and pain, etc., and thirdly, as the experiment continues, we can notice a group of sensations that are neither the one nor the other. These sensations may be described as if the muscle, if taken from the body and compared with one in a fresh condition, would feel softer, less resilient, less elastic and more shapeless. With this feeling there is, of course, associated the change in position as reported by the muscles themselves and by the tendons and joints. These sensations, so introspection further reports, would be decidedly pleasantly toned if it were not for the intrusion of the disagreeably toned pains and the drawing off of attention by the insistence of the feelings of effort.

Waller suggests that all three of these sensation groups are mediated by the same end-organs within the muscle. It seems more nearly correct to say that 'fatigue sensations' are just as often present with the other two as appearing at any definite stage between or after the pains and feelings of effort, as his theory would indicate. Furthermore, they seem to be the opposites, not of pain and disagreeably toned sensations, but of feelings of well-being and the condition of perfect possibility of action on the part of the muscles involved. The feeling of effort and the actual expenditure of effort as Mosso has shown by the ponometer and Trêves by his forearm ergograph are quite definitely correlated and they arise in a graded series

from the beginning of the experiment to its close. In other words, they form a conscious criterion of the general bodily energy in themselves.

Exhaustion.—3. The final stage of the feelings of effort, when they do not shade off into 'strains,' etc., is exhaustion, not fatigue or reduced capacity for work. Exhaustion is a phenomenon of complete loss of control and complete expenditure of effort where the fatigue sensations are almost entirely lacking throughout the experience. The above described conscious reports of fatigue are, thus, evidences of the normal demand on the part of the organism and the muscles in particular for a shift in the working mechanism.

Definition.—Fatigue presents itself in innumerable ways, but in general it may be said to express the situation underlying all regular and irregular rhythmic activity of the organism wherein the central point of that activity tends to shift toward other centers. Fatigue is conditioned by the functional interplay or 'excitement' that exists between muscular and nervous tissue. As a conscious phenomenon the locus of fatigue is primarily an activity of special end-organs lying within muscular tissue, referable apparently to 'deep sensibility' and the proprioceptive system of 'arcs.' Under this general characterization fall what we have termed automatic changes, rhythmic fatigue, conscious fatigue, sense organ fatigue, exhaustion, and fatigue, due to lowered metabolisms in the tissues. This statement attempts to express only the positive phase of the varied phenomena described.

Motor and neural relations of fatigue.—The phenomena as far as the experiments here reported can tell us are quite similar to the fluctuations that are found in the attention process, and in other mental processes where the control is exercised immediately by the 'strains' that exist in the paired muscles and by the organic sensations. These will be, if the unit group happens to be small enough, practically the same as though an external control were used. That is, the sensations will arise and will be located with all the definiteness that obtains when we say that our eyes or ears are tired from seeing or listening. When we use such motor controls as the hand or

the fingers or lips, the one difference that is found seems to be that the sensations that arise do not appear first in the muscles immediately concerned in the activity, *but in those parts that are more or less quiescent as far as actual noticeable movement is concerned, and are essential in the whole situation from the standpoint of 'strain.'* For example, in those muscles that control the eye in the visual reaction, or exercise control over the ear, we have the strain condition predominating over the actual work of the moving muscle. In such cases we have the fatigue sensations arising nearer the seat of the activity itself.

In movements that are run to their highest limit of speed as in the tapping tests, and all recording, we have similar results. No motor activity of a single more or less isolated group of muscles can approximate in rapidity the activity of the train of imagery.[1] When we come to piano playing, the use of the typewriter, and the larger groups of muscular activity, we are approximating the situation that we believe to obtain in a less (externally) striking way in the coördinations of shifting strains and tensions that control the mental activity of the individual. The speech organs furnish undoubtedly the best organized of all these definite, externally observable movements. *In all such systems of coördination, sensations of fatigue arise not in the muscle that is working under regular periods of contraction that are timed to suit the habitual rate, but in those portions of the muscle or muscles located at any place in the body that are under tension in guiding the activity of the muscles that are working, i. e., in the rigid muscles.*

In our historical review, we have seen that the present tendency is to refer the control of muscular activity to peripheral parts and to assign a more secondary function to the centers. The richness of the sensory end-organs in the muscles and the immense number of pathways leading from the conscious centers to them argues strongly for the reference of much more of our conscious life to the sensory stimuli arising from these sources. It is still true that the necessity for readjustment

[1] For a detailed discussion of the points involved see Burnett's and Woodworth's articles in the Garman Commemorative volume and an article by Miss Downey (27).

appears more frequently, at least in the adult, from without and through the ordinary avenues of the five senses. However, when this material seeks to become a part of our activities, and seeks to find its place among the coördinations already present, the fundamental importance of these sensory controls arising within the muscles begins to be apparent.

As indicated by the previous discussion, we would argue that the so-called mental fatigue has an origin that is also peripheral or muscular. In the first place, it seems reasonable if we are to state fatigue in terms of sensations at all, that we shall find some sensory origin for such sensations. The argument frequently advanced that there are functional nerve endings and possibly receiving end-organs in the central nervous system itself that mediate these sensations seems somewhat fantastical and at least out of place until we have exhausted the more probable sources. It is not necessary to argue that the sensations actually originate in the nervous system to be able to say that the nervous system contains the place at which the first functional shift takes place in fatigue phenomena. We may say with Sherrington and others that the blocking is due to actual changes in the synapses, the critical functional points for nervous impulses according to this author. The fatigue sensations and the decreased ability in activity, as shown in the muscles and in the increased mental effort, would still be concerned in breaking up the 'reflex' and 'sensory-motor' arcs.

Mental fatigue is of peripheral origin. If we limit this thesis to the 'sensations of fatigue' the statement would probably stand at the present time without much challenge. The majority of observers are willing to assign the origin of all sensations that can be isolated in any satisfactory manner to a sensory-motor control, that, in part of its neural pathway at least, lies outside the central nervous system. In fatigue and kinæsthetic sensations, these sensory endings are gradually becoming more and more definitely located as our knowledge of the anatomy of the body enlarges. In fatigue phenomena and elsewhere, it seems less and less acceptable to assign to the nervous system *intra se* functions that seem to be in all other

sense fields partly within the periphery. This is notably true in the highly detailed character of the analysis of eye movements that has led us to locate in the muscle coördinations many things that were previously held to be mental forms of an internal or metaphysical nature. So, for the fatigue that comes with a definite sense province, a special locus is gradually appearing as the analysis of the situation becomes clearer.

Even allowing that fatigue may possibly be a subnormal neural condition that is referred outward from the centers, we would still be compelled to assign some peripheral location to it. This holds good of 'referred pains' as well, and in fact the physician uses them merely as indications that point toward the real seat of the difficulty. In fatigue sensations he has a still better right to seek the seat of the difficulty in the periphery than at some central point, since here we know that certain parts are functioning and, in the majority of instances, we know exactly where those parts are located. This can be seen to apply without much difficulty in muscular effort, but it has not usually been considered as clear in the case of mental effort.

Two further points lead us to make the neuro-muscular hypothesis with reference to mental effort and the sensations accompanying it at all its stages. The first is that mental activity is not something superadded to the unconscious activity of an isolated muscle and differing in kind from it. So far as we know all conscious activity is accompanied by neural action, and in all organic life other than man we do not hesitate to assign definite correlations to the activity of neural and muscular tissue. 'Free imagery,' if there be such, would certainly be a distinctly new fact from the biological standpoint and constitute the entering wedge for any epiphenomenal doctrine of mind-body relationship.

The second point that leads us to make a peripheral statement of fatigue, both as a sensation and as a functional change, is connected with the close relationship that seems to exist between established motor coördinations on the one hand and types of imagery and mental processes on the other hand. There seems to exist in the naïve mind and in the experience

of the psychologist as well, certain well-defined situations that as often as they appear in consciousness are recognized by the individual experiencing them as being his, *his* way of reacting, *his* way of feeling toward the situation that has arisen, *his* method of attending to the process from without, *his* way of getting rid of the offending situation, and so on throughout the gamut of experiences. Ordinarily this peculiar subjective province has been bodily assigned to the aggregate of things called the 'feelings,' or more accurately the affections. But the analysis has gradually been pushed into the realm of these feelings and the so-called feeling mass seems to break up into certain definite sensation groups with their attendant pleasure-pain correlates. As regards these sensation groups we can, I think, push the analysis a step farther and show in some instances exactly what constitutes such a group.

Another relationship is brought out in the feeling of having experienced a situation at some previous time, so-called similarity feeling, or relational feelings. Dr. Dewey's (24) analysis of the feelings of effort starts us off in this direction. Effort becomes the conscious formation of a plan of action that has for its conscious basis the imagery of the memory processes both reproductive and productive. In addition it involves a very definite set of sensations arising from the muscles that were previously involved. We are here dealing with what seem to be real 'attitudes.' The major portion of the situation is made up of previous muscular coördinations and their correlated sensations and perceptions. This usual and habitual group finds its expression in what any observer finds to be a familiar feature in the reactions of our illustrative person, and which is recognized by the experiencing individual himself in certain introspective situations as his own way of responding and recognizing.

It seems to us that the control test for the analysis of this type of experience offers itself in the fatigue experiment. Here we can with ease detect at various times the shifts in the controlling tensions, the gradual elimination of some, with the rise of others, till we suddenly find by a backward comparison that the whole field of control has shifted to an entirely different

locus and that the attendant mental processes are something other than they were the few moments previous. We are dealing with the familiar shift of attention, in such a description. If the word 'attitude' is to have any meaning in psychology it must mean a group of fully coördinated elements that can find their locus in a common source. It seems anything but illuminating to find the term 'attitude' applied to such a heterogeneous mass of things as pleasure, pain, sensation states, interest, attention and so on, without seeking to find a common locus for the rise of these states (48). We should designate this common locus as the changes in strains and tensions arising from the musculature of the body.

Summary.—These motor attitudes, including in the term both sides of the process, the physiological and the psychological, are shifted by a special mechanism that presents its phenomena on the physiological side in three forms—inhibition and coördination, 'negative induction' or so-called muscular fatigue and fatigue proper, where the functional activity of the organism has diminished because of the attainment of a limit in the expenditure of stored-up energy. On the psychological side we may find three possible shifts in attitude that approximate these to some extent but not necessarily exactly, the reason being apparent in the differing functions assigned the two general fields. These shifts are produced by changes that are quite definitely 'in consciousness' in the formation of the focal points in the attention process. The first transition in attitude is made by those conscious correlates of the inhibitory and coördination processes that are called the 'feelings of effort'; the second by the 'sensations of fatigue' that are probable accompaniments of the process denominated by Sherrington 'negative induction.' As a special case, we have the conscious shift known as exhaustion, where the 'feelings of effort' have given place to their direct opposite. Consciousness expresses this situation in the absence of both 'efforts and strains,' and especially in the distinct loss of the preceding conscious process. In the third place, attitudes are modified and shifted by changes in stimulation, shifts from the sensory side of the process (adaptation and sense organ fatigue). The

primary functional coördination for all these would be the neuro-muscular relationship residing in the proprio-ceptive or deep-lying muscle systems. The secondary functional factors would involve the two divisions of the externally receiving system, the extero-ceptive and the intero-ceptive, both mediating sensory elements that in the latter are as yet psychologically unanalyzed 'groups.'

Sense stimulations that arouse sensations of fatigue are closely allied to the stimulations that usually set off habitual responses and therefore fail in many instances to get into consciousness at all except in the retrospective way in which we seek for the stimulus that begins habitual activity. This experience is so ordinary in its manifestations that its relation to the problem of fatigue has usually been overlooked, and as psychologists we have been accustomed to look for the cause of the inattention and restlessness in images and external stimulations. As a habit, then, fatigue constitutes a highly specialized form of protection for the organism and enables the normal individual, who has not converted this habit into a means of avoiding effort, to gauge his activity within very definite limits.

This generalization is quite comparable to the situation described in part by Claparède[1] as leading to the appearance of the instinct of sleep. Allow the sensations of fatigue to arise undisturbed by a necessary end, the idea of a task that is incomplete, and the instinct that is most closely connected with it appears. The introduction of the feeling that the work must be done, together with the emotional element that accompanies this conflict of the biological and the psychical, causes a new condition to arise, which is psychical fatigue.

[1] Both Féré and Joteyko have argued the biological significance of fatigue at some length; its correlation with the phenomenon of sleep has been mentioned in some detail by Claparède.

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THE DETERMINATION OF THE POSITION OF A MOMENTARY IMPRESSION IN THE TEMPORAL COURSE OF A MOVING VISUAL IMPRESSION.

I. Historical Survey.

Herbart originated the use of the word 'Complication' to designate the conjunction in consciousness of dissimilar impressions. Wundt, following Herbart's usage, applied the term 'Complication Experiment' to the specific investigation of the perceived temporal relation of a single impression of one sense to a continuous series of impressions of another sense, into the course of which it is suddenly introduced.[1] For the continuous series of impressions Wundt employed visual stimuli, and for the disparate impression an auditory one. The general conditions of Wundt's experiment present a situation analogous to that obtaining in the so-called 'eye and ear' method of the astronomers, to which it owes its inception; and his research presents, under conditions which subject it to accurate psychological measurements, a problem similar to that with which the astronomers were early confronted.[2]

In the use of the transit instrument, the field of view is divided vertically by a series of equidistant threads, the central one of which represents the meridian. The astronomer, who wishes to note the moment of a star's transit, *i. e.*, passage across the meridian, observes the time given by a clock beating seconds. Then, his eye at the telescope, he counts the beats of the pendulum, to determine the second at which the star coincides with the meridian line. Since it seldom happens that the star is coincident with the line at the precise moment of the pendulum stroke, the additional fraction of a second

[1] WUNDT, *Grundzüge der Physiol. Psychol.*, 4th ed., ii, pp. 401–; 5th ed., pp. 67–86.

[2] For a historical account of the "personal equation," see SANFORD, *Amer. Journal of Psychol.*, vol. ii, pp. 3–38, 271–298, 403–430.

is estimated by noting the position of the star at the stroke just preceding and at the stroke just following its passage across the meridian-line, and "dividing the time as the meridian-line seems to divide the space." Notwithstanding that a rigid control attempted to eliminate errors from every possible source, discrepancies invariably occurred in the computations of the time at which the star crossed the meridian, owing to the personal difference between various observers, the error frequently introducing a variation of more than a second in the readings. Bessel, who first noted this factor, gave to it the name of 'personal equation.' The analogy of the eye and ear method to the complication experiment will appear from a consideration of Wundt's procedure.

The apparatus which Wundt first had constructed for the purposes of his experiments, he called the 'complication-clock."[1] It consists of a graduated circular dial, placed vertically and traversed clock-wise by an index-hand. The mechanism is that of a large clock, run by a weight, and having a fan-regulator for maintaining constant velocities, which however, may be arbitrarily varied within limits. It is so constructed that a bell-stroke may be produced coincidently with the alignment of the index-hand with any chosen division of the scale on the dial, the point selected on the scale being unknown to the observer. The index-hand of Wundt's apparatus was 25 cm. in length, equal to the radius of the dial measured to the proximal end of scale divisions; *i. e.*, the index-hand extended outward only as far as the scale divisions, so that in rotating it did not pass over them but moved by their inner ends.

Comparing now the elements in the mechanism of the complication apparatus with those of the eye and ear method of the astronomers, it is seen that the equidistant threads dividing the field of view of the transit instrument correspond to the series of scale divisions on the complication dial, that the star is represented by the index-hand, while to the pendulum-stroke of the clock beating seconds corresponds the auditory

[1] For a complete description of Wundt's complication-clock, see GEIGER, *Philos. Stud.*, vol. xviii, p. 349.

impression entering regularly into the course of the series of visual impressions of Wundt's complication apparatus. From this comparison it is clear that the errors in the estimated time of coincidence of star and meridian are analogous to the time-displacements occurring in the attempt to judge the position on the scale at which the passage of the index-hand is synchronous with the bell-stroke, as presented in the complication experiment. Later, Wundt substituted for the complication clock, with its uniform rate of rotation, a form of apparatus with index-hand actuated by the swinging of a pendulum, with a view to introducing the factor of varying acceleration.

The second form of apparatus, as distinguished from the complication-clock, he called the complication-pendulum. As in the complication-clock, the position of the index-hand (with regard to the scale), to which the bell-stroke corresponds, may be varied at will; but there is a difference in the movement of the index-hand, in that, as it now depends upon the swinging of a pendulum, it no longer rotates as before, but oscillates back and forth across the upper half of the dial, the instrument being so contrived that the bell-stroke occurs only when the index-hand is moving in the direction corresponding to that of the hands of a clock; *i. e.*, from left to right. The pendulum is at the center of its swing when the index-hand is in alignment with the upper vertical radius of the dial, to the right and left of which, depending upon the direction of movement, it is accelerating or retarding, in accordance with the law of harmonic motion. The radius of the scale and of the index-hand, which traverses the inner margin of the scale as in the original form of apparatus, is 17 cm. The scale is a half-circumference in extent.[1]

Wundt was himself 'subject' throughout his experiments. When using the clock-machine, he stood in front of the dial, which was concealed by a wooden screen until the index-hand had attained the desired speed. He adopted no rigid method of observation, but in a manner as free from prejudices as possible, awaited the moment at which, after a sufficient num-

[1] For full description of pendulum-apparatus see WUNDT, *Physiol. Psych.*, 5th ed., vol. iii, p. 80.

ber of revolutions, he felt secure in naming the position upon the scale at which the index-hand appeared to be at the stroke of the bell. He noted this point; then, stopping the machine, compared his judgment with the actual position of the bell-stroke, which was determined by slowly rotating the index-hand until the bell-stroke occurred. Then he fixed the position for the next experiment, making the change carelessly, so that he did not know the new position. The rates of rotation which Wundt used varied between one revolution in a little less than 2 secs. and one revolution in a little more than 8 secs. When the index-hand revolved at a rate much slower than 8 secs., its position was clearly visible; when much more rapid than 2 secs., it was not possible to obtain a judgment.

In the outcome of his enquiry, Wundt found that correct judgments were the rare exception; that is to say, that it was seldom, and then apparently by accident, that the subject's *feeling* of the simultaneity of the sound with a given position of the index-hand agreed with the *actual* simultaneity of the two impressions. The subject selected a position on the scale at which the index-hand arrived either after the sound occurred or before it occurred. That is, in the former case, the sound impression was as if too tardy, the index-hand having already passed the assigned position, whilst in the latter case, the sound impression was as if too premature, the index-hand not yet having had time to reach this position before the sound was perceived. These two classes of error Wundt called arbitrarily *positive* and *negative;* positive errors occurring when the assumed position of simultaneity is later than the real one, and negative errors when the assumed position is earlier than the real one. Wundt found, contrary to the natural expectation, that negative errors were largely preponderant, it being the prevailing tendency to anticipate the sound, as it were.

Among the factors modifying the results, Wundt regarded the influence of speed as of chief importance. This is manifested in the difference in the quality of the error, according as the rate of rotation is slow or rapid; the negative errors predominating with slow speeds, whilst with greater velocities the positive errors prevail. At a certain point between the ex-

tremes of speed the average error becomes zero. This indifference point Wundt found to lie within the range of rotation-rates from 5 to 2 secs. With the complication-pendulum, with which he operated in the subsequent experiments, Wundt obtained results which agreed in the main with the preceding. The especial feature which Wundt noted in the use of the pendulum apparatus was a characteristic tendency to more negative errors if the bell-stroke occurred during a phase of acceleration of the index-hand (as was the case when moving from the left upward toward the median-line of the dial) and the tendency to more positive errors when the bell-stroke occurred during a phase of retardation of the index-hand, (as when moving downward from the median-line to the right-hand limit of the scale).

W. von Tschisch, who succeeded Wundt in experiments upon the complication problem, also made use of the pendulum machine.[1] He too was 'subject' for his own experiments. His results, in the main, corroborated Wundt's, the negative errors becoming less negative with increasing speed; but his average absolute errors were invariably negative, when experimenting, as Wundt, with a single complicating stimulus. The rates of oscillation of the pendulum, and therefore of the index-hand, used by von Tschisch, were one oscillation in 2 secs., in 1.5 secs., and in 1 sec.

The chief interest of von Tschisch's investigations consists in the substitution of other complicating impressions for the auditory impression of Wundt, and in the combination with sound impressions of other stimuli and of these in varying ways among themselves. For example, employing a touch-impression instead of sound, the results showed no noticeable change. However, where one or more disparate impressions were simultaneously combined with sound, the negative error was made smaller, and the error shifted progressively in a positive direction in proportion to the number of disparate stimuli which were thus combined. When three disparate complicating stimuli were employed, the error was positive.

[1] Ueber die Zeitverhältniss der Apperception einfacher und zusammengesetzer Vorstellungen. *Philos. Studien*, ii, pp. 603–634.

The occurrence of two disparate impressions within the same sense realm, but differing in quality, as the stroke of a bell and that of a hammer, have the like effect of diminishing the negative error. Von Tschisch further observed that the diminishing effect upon the negative errors exerted through adding to the number of momentary simultaneous impressions, was far less under conditions which admitted of an association between the impressions in question; *e. g.*, stimuli upon closely adjacent areas of the skin, wherein the touch impressions are united in a single space concept. If instead, widely separated areas of the skin be stimulated, the subject reacts as if disparate impressions were employed.

The next investigator in the field of complication-work was C. D. Pflaum.[1] He too used the pendulum form of apparatus. He experimented on himself and on two other subjects. His results concur with those of the two former Leipsic experimenters, *i. e.*, they show a preponderance of negative errors, which with increasing speed manifest a positive trend. The rates of pendulum oscillation employed by Pflaum were 2, $1\frac{2}{3}$, $1\frac{1}{2}$, and 1 seconds per oscillation.

It may be said here that all of the above experimenters are in harmony in respect to the essential returns. All are in accord as to the importance of the factor of the rate of rotation in determining the quality of the errors, the tendency to negative errors predominating with the slow speeds; and the gradual increase of positive errors with increased velocities. The experiments of both Wundt and von Tschisch number in the thousands, those of Pflaum number several hundred.

We next come to consider the studies upon the complication phenomena which were carried on in the Harvard Laboratory by J. R. Angell and A. H. Pierce.[2] These investigators resorted again to the method of uniform velocities and for their purposes constructed an apparatus similar in principle to the complication clock of Wundt. An ordinary kymograph served

[1] Neue Untersuchungen über die Zeitverhältniss der Apperception einfacher Sinneseindrücke am Complicationspendel. *Philos. Studien* xv, pp. 139–148.

[2] Experimental Research upon the Phenomena of Attention. *Amer. J. of Psychol.*, iv, pp. 520–541.

as the motive power. The chief difference between it and the older instrument consisted in the substitution of a sharp click for the reverberating bell-stroke, and of a dial which was but a portion (about $\frac{1}{6}$) of a circumference. The arc of the circle represented by the dial was below the center, its middle point lying on the lower vertical radius of the circle, so that as the index-hand revolved in the direction of the hands of a clock, its direction, when crossing the dial, was from right to left, and the index-hand, when approaching the center of the dial, moved in a downward course, just the opposite of the course of the index-hand in the complication pendulum, in which the arc of the circle traversed was above.

The index-hand which Angell and Pierce employed was much longer than the index-hand of the Wundt machine, measuring 47 cm. from center to tip. The complicating stimulus consisted of a sharp click produced by an electric hammer. The circuit through which the hammer was actuated was closed by a wire fastened to the index-hand and dipping into a mercury cup behind the scale. The position of the mercury cup along the scale was variable, and hence the sound could be fixed for any desired position of the index on the scale. In method, Angell and Pierce departed from the hitherto customary plan of leaving the subject quite free and untrammeled as to his procedure and adopted a usage which was, in their own words: "to follow the pointer on its first revolution until the sound is heard, when we attempt to stop the movements of the eyes instantaneously. The point on the dial thus attained is made the basis of operation for the next revolution, when any seemingly needed correction is made." They chose three rates: a slow, a medium, and a rapid rate, representing one revolution in 6.06 secs., 3.6 secs., and 1.2 secs. respectively. Angell and Pierce had presumably but few observers, as their experiments amounted to a comparatively small number. Their results show a wide divergence from those of the preceding German experimenters. In the first place they find on the whole, in contradiction to Wundt and his followers, a preponderance of positive errors appearing in the results of each subject toward the end of their series of experiments, which

indicates in the light of their interpretation the presence of a hitherto unrecognized factor, namely, that of practice. In the second place their results were further at variance with those of the preceding investigators in that they discovered no influence of velocity. The factor of varying acceleration did not come into court, since they worked with uniform rates of rotation.

In a comparative survey of the results of the two sets of investigators and of the conclusions reached by them with regard to the basic factors operative in producing the errors in the complication experiment, it appears that the German experimenters find a preponderance of negative values, while contrariwise the Americans find that the tendency to positive errors is the prevailing one, at least after practice. Further, the German investigators find that the negative values predominate with the slower speeds and the positive with the faster. With them no influence of practice is manifest. Angell and Pierce on the other hand deny the influence of speed in determining the quality of the judgment, but emphasize the influence of practice, the effect of which is gradually to shift the errors from the negative direction toward the positive. So that the conclusions of Angell and Pierce, as to the result of their inquiry were: influence of practice and no influence of speed, and of the Leipsic experimenters: influence of speed and no influence of practice. As said above, Angell and Pierce used constant rates of rotation, so that the question of the influence of varying acceleration inseparable from experiments with the pendulum apparatus did not enter into their determinations.

Such was the conflict between the returns of the Leipsic and the Harvard investigators until M. Geiger, working in Wundt's laboratory, attacked afresh the problem of complication.[1] This investigator reverted to the original type of apparatus, represented by Wundt's complication clock, but he departed from Wundt's procedure, in abandoning the use of the wooden screen for concealing the apparatus in the intervals of experimentation, preferring simply to disconnect the sound-stimulus

[1] Neue Complicationsversuche. *Philos. Studien*, xviii, pp. 347–436.

instead, as the repeated raising and lowering of the screen tended, in the opinion of this experimenter, to confuse the subject and vitiate the results. He also substituted for the bell a sharp hammer stroke, as the reverberations of the bell-stroke failed to satisfy the requirements for a strictly momentary auditory impression, and so did not fulfill the conditions essential to the experiment.

Since in the beginning of his work Geiger's method was directed by the purpose of a series of experiments serving as a control upon the results hitherto attained, he at the outset sought to eliminate the coefficient of individual peculiarity and to this end enrolled seven observers for the conduct of his experiments, a larger number than had been employed by any previous experimenter. With a view to isolating the influences of practice and of speed as factors determining the quality and magnitude of the errors, Geiger began with each observer with a speed of one rotation in 8 secs., and with successive sittings progressively decreased the time of rotation by half-second decrements to 1 sec., and finally to 0.9 sec., the extreme of speed to which his apparatus was adjustable. He then reversed the proceeding, beginning with a rotation-rate of one revolution in 0.9 sec., increasing the time to one revolution in 1 sec. and from here upward by ½ sec. increments till a rotation-time of 8 secs. was again reached.

Geiger, whose work, as indicated, is primarily a control upon the conflicting results of his predecessors in the field, reconciles, in the main, the contradictory aspects presented by the conclusions of Wundt and his pupils on the one hand and those of Angell and Pierce on the other. He shows that both factors are operative—the influence of speeds and the influence of practice.

Having proceeded as above described, Geiger constructed a curve that traces the effect of his first series of experiments, that namely in which he used gradually increasing rates of speed, to illustrate the joint influence of practice and of increasing velocity—the two determinants going hand in hand in such a series. The curve thus derived represents a gradual progression from a markedly negative beginning to a termination upon the positive side. Now if the conclusion of the Leipsic

experimenters is correct and the positive tendency in the above series is due to speed alone, then the tracing of a second curve drawn from comparison and derived from a precisely contrary proceeding, *i. e.*, setting out with the fastest rate and gradually passing over to the slowest, ought to proceed in a direction just the reverse of the former, and represent an approximate retracing of the first curve in the opposite direction. But if, on the other hand, the assumption of the American observers is correct and practice alone is the decisive factor, the continued practice in operation after reversing the direction of change of velocities ought, on the contrary, to be represented by a curve that mounts still higher in the positive direction, continuing the course of the original curve. Now the curve actually resulting in the case of each of Geiger's subjects is found to be consistent with neither assumption, but is clearly the resultant of both forces —namely the influence of speed and the influence of practice; for the second curve is neither a reversal nor a continuation of the first, but exhibits a trend which in the main fluctuates between the influences of the two factors, the tracing for some observers tending feebly toward the positive, in others as faintly toward the negative side. True, there is a slight partiality toward the latter course, since in both curves the influence of speed is uniformly operative throughout, while from first to last the influence of practice is a decreasing factor and with the commencement of the second curve its efficacy has considerably waned.

With one of Geiger's subjects the influence of speed is shown in isolation. Here the succession of speeds proceeds irregularly, yet the rise of the curve coincides with the increased velocities. Further the influence of practice is manifest in individual experiments in which it happens that speeds which give an error of four or five scale-divisions, later reduce the error to nil. According to Geiger, the influences exerted by practice upon the errors are chiefly two. 1. With constant velocities increasing practice makes the errors constantly less negative or more positive. 2. Practice causes the maximal negative error (or minimal positive error) to occur at a speed greater than that at which it previously occurred.

From the above experiments, it is evident that Geiger's results are in full accord neither with those of the Leipsic nor with those of the Harvard observers. Geiger seeks to explain the discrepancy between Wundt's results and those of Angell and Pierce and himself with regard to the influence of practice, on the ground of the difference in apparatus employed. The pendulum-apparatus which Wundt used precludes by reason of its constantly varying speeds the possibility of effects of practice at any speed, while, on the contrary, the rotation device of Angell and Pierce and the complication-clock of Geiger favor the introduction of such effects through the uniformity in the movement of the index-hand. The explanation of the divergence of Angell and Pierce's results from Wundt's and his own in their failure to detect any influence of speed may be more fittingly stated after the consideration of the other constituents which Geiger includes in the sum of factors entering in to determine the results.

As a factor influencing the magnitude and the direction of the errors, Geiger lays stress upon the difference in the manner of observing characteristic of different individuals and accordingly distinguishes two types of observer, which he calls, for convenience, '*naive*' and the '*reflecting*,' and names the corresponding methods, the 'index-hand' and the 'scale' method of observation. Characteristic of the 'naïve' or 'index-hand' method is the passive, quiescent, leisurely attitude of the subject. Quite indifferently he permits the impression of the *region* of probable simultaneity of sound and index-hand to arise within him, and later, with successive rotations, he selects, within this region, or rather there selects itself, as it were, the position at which the index-hand and the stroke are synchronous. The observer of the 'reflecting' type on the contrary is tense and alert. He is bent upon catching as quickly as possible the exact position of the index-hand at which the sound enters. He does not wait composedly until, after several rotations of the index-hand, the apparent region of simultaneity is established, but seeks to determine its position upon the scale at the first entrance of the sound. He now holds strongly to the scale-division thus selected and with

the next revolution fixes it by the reading on the scale and then tries further to confirm his choice with the succeeding revolutions, or if unsuccessful, to make the needed correction, depending upon whether the sound appears to come before or after the point selected. Thus it is seen that careful thought and more or less of computation enter into the determination of the resulting judgment and give the cue for designating it as of the 'reflecting' type.

The different attitudes of the two types of observers toward the judgment made upon the first revolution of the index-hand is significant. One of Geiger's subjects who was of the 'reflecting' type, records that if he succeeds in getting a judgment on the first rotation, he has a feeling of security as to his determination, but Geiger himself, whose method was 'naïve,' finds the first judgment (the judgment obtained upon the first rotation of the index-hand,) relatively of no importance for his subsequent determination.

It is noteworthy that the 'naïve' subjects were able to observe according to the 'reflecting' method when directed to make the attempt, while the reverse was not readily the case. A further point of differentiation between the opposed types is to be seen in the importance of the rôle which accidental fixations of the attention play in 'naïve' judgments, as evidenced by the change of reading with successive rotations—each such change ranging over an interval of as many as 3 or 4 scale-divisions.[1]

Geiger's results show that for those of his observers whom, upon the basis of their method of procedure, he had classified as 'reflecting', the two curves above mentioned lie for the greater part upon the positive side, while those belonging to the 'naïve' type presented curves which lie upon the negative side. With the subjects in whom Geiger found neither type characteristically presented, but in whom the method consisted in a mingling of the two procedures, the curves follow courses which in general are intermediate to those of the distinctly discernible types. Since in the light of the method of experimentation employed by Angell and Pierce their procedure

[1] The scale was divided into 100 parts

clearly corresponds to the 'reflecting' type described by Geiger, he refers the preponderance of positive results shown by them to their experimental usage and traces to this source the causes of the discrepancy between the returns of these experimenters and those of Wundt. However, in seeking to base an explanation of the negative errors of the Leipsic experimenters on a reverse procedure, the clue did not prove to be the correct one, for in cases in regard to which information was obtainable, Geiger learned that the procedure adopted by these investigators included both methods, so that he was obliged to seek for other explanations for the peculiar results of these experimenters, and finally ascribed the difference of result to the different apparatus used. For obviously, owing to the difficulties attendant upon the inconstancy of the velocity which is a condition of the pendulum apparatus, the factor of practice is eliminated, and the two methods of observation are no longer characteristically present, but (Geiger maintains) lose their specific qualities and merge more or less into one another. We shall return to the question of temperamental types in a further connection.

It has been shown that increasing speeds of rotation increase the positive tendency of the errors. But increase in the rate of movement of the index-hand goes hand in hand with increase in the rate at which the momentary or disparate impression occurs. Hence the question arises: To which of the two factors is the result attributable? In the 'eye and ear' method of the astronomers it has been observed in some cases that positive errors increase with the more rapid course of the star[1] and in this case of course the rapidity of the disparate impression (clock-tick) is unchanged. It had also been shown by Bessel that altering the rate of the clock beat alone might change the error in the observation of the star's transit. Geiger attempted to investigate the influence of altering the rate of the successive strokes of the sound alone, the visual impressions occurring at the same rate. This he did by taking a series of experiments in the usual way and alternating with a series in which the hammer-stroke occurred only at alternate

[1] Sanford, *Op. Cit.*, pp. 287–8.

revolutions of the index, so that the rate of auditory rhythm was decreased to just half the original rate. In his account of this variation of his experiments, Geiger is unsatisfactory. He says at one place that the effect of omitting the alternate strokes is a greater positive tendency, at another that it is a less. Further, his final statement is not in accord with his tables. This method of determining the influence of the slower auditory rhythm is inadequate at best, for in thus introducing two revolutions of the index-hand for each auditory stimulus, Geiger arbitrarily neglects the factor of the alternate blank rotation of the index-hand and of the possible effect of this upon the attentive process (in deflecting the judgment to one side or the other). Moreover there is no certainty that the actual rhythm is changed at all by this method, since the interval between two strokes at the slower rate may be broken up into two attention-periods, corresponding to the periods of the fast rate.

An interesting point first brought out by Geiger is the factor of positional error, dependent upon the position of the index-hand at the moment of the auditory stimulus, in one case on the right hand side of the scale, in which it has a downward direction, and in the other case on the left hand side, in which position the direction of its movement is upward. The experiments which Geiger undertook to test the influence of positional error show with remarkable regularity that the deviation of the average error upon the right side of the scale from the general average error is in the positive direction, and upon the left side in the negative direction, the maximal negative error in the latter case lying toward the beginning of the upward course of the index-hand, while the positive influence upon the errors tends to a more uniform distribution upon the corresponding side of the scale.

Von Tschisch earlier called attention to this difference in the tendency of the errors upon the opposite sides of the dial, but he attributed the source to the acceleration of the index-hand, characteristic of the pendulum-machine, when moving upward as on the left side, and to the retardation when moving downward as on the right. Apparently von Tschisch

was upon the wrong track, since Geiger observed the influence of the same factor with the uniform velocities of the complication-clock.

Wundt and Pflaum, who, as has been said, made use of the pendulum machine, also performed experiments which showed the stronger negative influence of the upward moving index-hand when the upward movement corresponded to the phase of acceleration, or the stronger positive influence of the downward movement, when this coincided with the phase of retardation of the index-hand. But these observers failed to discriminate severally the two factors of which their determinations expressed the resultant. The attempt at the differentiation of the factors here involved was undertaken by a later investigator who will be mentioned below. The problem with which Geiger was concerned had to do with the simpler conditions presented in the clock machine, in which the factor of change of acceleration was eliminated, allowing the isolation of the influence of the different directions of motion on the lateral halves of the dial.

In order to rule out a possible defect of the apparatus which might operate in such a way as to cause an actual difference in the movement of the index-hand upon one side or the other, Geiger obtained an inverted image of the dial by interposing a telescope between it and the observer, the observer's visual field being exactly coterminus with the dial. Thus the uppermost and lowermost points being interchanged, the index-hand now moved in a downward course toward the point which before was the top of the dial and in an upward course toward the point which before was the bottom. That is to say, the image of the hand moved down when the real hand moved up, and *vice versa.* Notwithstanding the relative reversal of the positions of the dial in regard to this upward and downward direction of movement of the index-hand, the results were unaltered—the errors being more negative on the side of apparent upward movement, or more positive on the side of apparent downward movement, thus clearly excluding the possibility of irregularity in the mechanism of rotation as a cause of the difference in errors upon the two sides of the scale.

Geiger subjected to a similar test the influence of a possible predilection of the subject for the right or left side in causing the difference in the quality of the judgments, by reversing the two sides. This he did by having the subject observe the image of the dial reflected in a mirror. Again the results were substantially unchanged, showing that the direction of rotation from left or right was inconsequential.

Finally, Geiger proceeded to test the influence of the peripheral factor of eye-movements occurring concomitantly with the upward and downward movement of the index-hand. To this end he experimented in a darkened room and by replacing the original form of index-hand and scale-divisions with slits in a black surface and made visible by light entering from behind, he was able to observe the relative breadth of the after-image obtained during movement of the index-hand in the two directions. The results showed an after-image of greater extent upon the side of upward than upon that of downward movement, thus introducing a condition favorable to more negative readings upon the side of the ascending direction of the index-hand, as compared with those of the descending.

While Geiger disentangled the elements of acceleration *versus* retardation upon the one hand (as presented in pendulum apparatus) and of upward and downward movement of the index-hand upon the other and traced the source of the latter to the relatively easier downward than upward movements of the eyes, Klemm working with the pendulum,[1] further isolated the two factors by alternately transposing the conditions of experimentation in such a manner, that the upward movement of the index-hand of the complication-pendulum was now coincident with retardation of the velocity, while conversely the downward movement was accompanied by an acceleration of the index-hand. This interchange of the relations of acceleration and retardation to the upward and downward movements of the index-hand, Klemm effected by inverting the medial position of the index-hand. This being

[1] Versuche mit dem Komplicationspendel nach der Methode der Selbsteinstellung. *Psychol. Studien*, ii, pp. 324–357.

in the original apparatus above, it was now below the center of the scale (the index-hand of Klemm's apparatus described nearly a complete circle), and therefore as the index-hand now approached the mid-point of the scale it moved in a downward course, with an increasing velocity: and as it left the mid-point it moved in an upward direction with a decreasing speed.

Now upon comparing points on the upward path of the index-hand in the transposed arrangement with similarly situated points on the downward path in the original arrangement, and *vice versa*, thus entirely excluding the influence of acceleration or of retardation, he found that the preponderant negativity still obtained upon the side of ascent; and upon a comparison of points upon the paths in the two arrangements, which points were identical as regards direction of movement and velocity, but at which was presented in the one case a phase of acceleration and in the other a phase of retardation, Klemm still found coincident with the phase of acceleration a greater negative tendency of the error as compared with the tendency occurring with retardation.[1]

The especial interest of the experiments undertaken by Klemm lies in the altered method of procedure whereby the observer himself places a given mark in the position on an otherwise unmarked dial, at which the index seems to be when the sound enters. (The fundamental apparatus he employed is a modification of the original Wundt complication-pendulum.) In essentials the auxiliary device which this investigator used consists of a mirror, placed in front of the observer, between him and the dial. A circular opening through the mirror permits the observer to follow the movement of the shaft of the index-hand upon the dial, while the end of the index-hand, together with the scale is concealed from view by the mirror. The observer looks through a small circular opening in a screen, behind which he is seated. Upon the face of the screen opposite the mirror is a white annular surface the size of which corresponds to the graduated circumference of the dial, which

[1] It will be pointed out in Pt. III that Klemm's results on this point by no means indicate that the differences are really due to retardation and acceleration.

(the mirror being placed in a position equidistant from the dial and the white annular surface), is directly replaced by the reflection of the white ring—it being understood that the line of vision is perpendicular to the plane of the dial, reflected ring, mirror, its annular opening, and the hole through which the observer looks, at their centers. A second index-hand, called the marker, which may be moved radially over the white ring by means of a lever placed correspondinglv to it upon the back of the screen, is thus under the control of the observer. With this arrangement therefore the observer who, through the opening in the mirror, follows directly the movement of the index-hand, may indicate its position in relation to the complicating stimulus, by adjusting the reflected marker to the position in which it is in alignment with the index-hand at the stroke of the sound-impression. Klemm used only two rates, 1.2 sec. and 2.1 sec. per oscillation. His farther problem was the determination of the width of the 'Simultaneitätsbereich;' the simultaneity zone, or region within which all positions are practically simultaneous with the discrete stimulus; and its relation to the average deviation or error. The main point brought out in this regard is the extreme variability of the region in question.

A simple complication-apparatus was devised by H. C. Stevens[1] through the use of a bell metronome. A piece of white card-board is fastened to the instrument in such a way that the pendulum oscillates in front of it. The card-board presents the arc of a circle, the radius of which is equal to the length of the pendulum, scaled in 5° intervals. The position of objective coincidence of the bell-stroke and pendulum, may be ascertained as in Wundt's machine, by slowly moving the pendulum across the dial until the bell sounds. The method of Stevens' experiments with this apparatus requires that the subject shall at one time direct his attention to the sound, in which case the sound appears to come *before* the position of objective coincidence (negative error), and at another that he direct his attention to the position of the pendu-

[1] A simple Complication Pendulum for Qualitative Work. *Amer. Jour. of Psych.*, xv, 1904, p. 581.

lum, in which case the sound appears to come after the real positions (positive error). This modification of method Titchener[1] calls the 'inversion' of the complication experiment, because in it the subject is not left unbiased but is expressly instructed to which member of the two simultaneously occurring stimuli he shall voluntarily direct his attention.

T. H. Haines[2] took up the complication problem in a new way. His apparatus consisted essentially of a black disc on which were mounted at regular intervals white letters, which, by the rotation of the disc were brought in succession before the field of view of an observation tube. This series of visual stimulations was complicated with an auditory impression produced by the stroke of an electric hammer. The circuit for this hammer was completed through a mercury cup by a copper wire attached to a wooden arm on the rotating shaft which bore the disc.

Haines used various rates of succession of the letters and of the sounds, varying them together and independently. His results agree in part with the Leipsic rule of change of average error with rate, and with the Angell and Pierce rule of change with increasing habituation. The fact that no correction was made for the errors in the apparatus, which were probably large at certain rates, makes the interpretation somewhat insecure. The method, however, seems a promising one.

Certain observations were common to all the experimenters. All noted the significance of accidental combinations of the attention in determining the judgments. Certain scale-divisions seemed to exercise a peculiar fascination upon the attention, and Wundt noticed that the attention invariably held to the scale-divisions, even when the actual position of entrance of the sound was made to occur midway between them and in cases in which the scale-divisions were separated by a considerable interval. Wundt performed experiments in which he concealed from view all but a single scale-division, and setting the position of the sound at points on one side and the other, he obtained errors as great as $\frac{1}{4}$ sec. in the time-displace-

[1] *Psychology of Feeling and Attention*, p. 252.
[2] *Harvard Psychological Studies* 1906, vol. ii, pp. 309–348.

ment. Geiger experimenting similarly obtained errors of as much as $\frac{1}{10}$ sec. It was also the general observation that a single revolution of the index-hand is inadequate to insure a conclusive judgment. For this it was necessary that a sense of rhythm in the succession of the momentary sound stimuli establish itself; so that several rotations were often requisite before the mind felt prepared to determine its choice. The difference in the number of preliminary rotations required, Geiger found to vary for his subjects between as few as three and as many as forty-three revolutions. Pflaum observed that the number of preliminary rotations required increased with the more rapid rates. If an increase occurred while the speed remained constant, it was an indication of the onset of fatigue.

II. NEW EXPERIMENTS AND RESULTS.

In the work just reported the assumption has been implicitly and sometimes explicitly made that the time-displacement of the discrete stimulus in relation to the series of other stimuli is essentially a complication phenomenon, *i. e.*, that it depends upon the introduction of a disparate impression into the series.

In the paragraph of Wundt's Physiologische Psychologie introductory to his description of the complication experiment,[1] he applies the name 'time displacements' to such time illusions as occur when simultaneous sense impressions or sense impressions but slightly discrepant in time appear to be displaced in relation to each other in such wise that impressions which are in truth simultaneous are perceived as successive, or where a succession is actually present, the impressions are perceived as simultaneous or even transposed so that the earlier is perceived as the later and the later as the earlier. But Wundt insists upon the essential disparateness of the two impressions as the *sine qua non* of such displacements and there is in the text the obvious implication that this stipulation applies equally under the specific conditions of the complication experiment. "Such time-displacements," he says, "occur essentially between disparate sense impressions, that is, between visual and audi-

[1] 5th ed., p. 4.

tory, visual and tactual, and between tactual and auditory. They happen, when the impressions are within the same sense-realm, only when the impressions affect different individual organs, as the two eyes or ears, or positions upon one and the same organ which are widely removed from each other, for example, upon the skin of the upper and of the lower extremity. That stimuli upon the same ear or eye, or upon one and the same position of the skin should be temporally displaced with regard to each other, is, on the contrary, never to be observed."

Wundt then attributes the chief source of the phenomenon of time-displacements to the preference of the attention for stimuli received upon sense-organs in which the physiological transmission is the more rapid, claiming upon this ground, that the attention seizes upon an auditory impression more readily than upon a visual one, because of the conditions for the quicker reception of a stimulus in this sense organ.

He then argues that the sense organ in which stimuli are more favored by the attention is, *de facto*, the sense organ which will find most favor in the process of 'apperception.' It was primarily with a view to testing the validity of the above quoted statement of Wundt, and the explanation of time-displacements hinging upon it, together with the *a priori* assumptions Wundt adduces in support of it, that my experiments were undertaken.

To subject to a critical experimental test the question whether time-displacements may occur where the two impressions belong to the same sense organ and are received upon adjacent portions of it, I substituted for the disparate auditory stimulus entering momentarily into the course of the series of visual impressions as presented in Wundt's complication experiment, a momentary visual impression.

Before describing the technique of my procedure, I wish to call attention to certain features of the apparatus I employed, in which it possesses a distinct advantage over the instruments hitherto used.

As I have said, the dial of Wundt's machine was marked with scale-divisions. Those were clearly seen by the observer

and by means of them he indicated his judgment of the position. In another arrangement, which Geiger adopted for the purposes of a variation of his experiment, the index-hand traverses a surface which is unmarked throughout its extent. Obviating the objections to which both these arrangements are open, a characteristic feature of my experiment is the introduction of the device whereby the subject may himself shift the temporal position of the discrete stimulus until it appears simultaneous with the alignment of the index-hand and a determinate mark or goal upon an otherwise undifferentiated dial. It will be recalled that Klemm employed the method by which the subject uses an adjustable marker to denote the position at which he judges the two impressions to be simultaneous, but by this method of self-fixation it is the marker which is brought to the position of entry of the discrete stimulus. This is doubtless a convenient method of locating the position of subjective simultaneity, and permits the determination of the limits of the region of simultaneity, but it does not otherwise alter the essential conditions of the experiment.

My apparatus had a semicircular dial, upon which appeared a single black mark (called the *goal*), placed at the middle point. The pointer moved over this dial and by this mark at uniform rates, while the temporal position of the discrete stimulus with regard to the series of positions of the index-hand could be varied at will by the movement of a lever. The subject was required to fix his eyes on the goal and vary the aforesaid temporal position, until it coincided with the alignment of index-hand and goal, starting alternately from positions much earlier and much later.

The advantage of the method I adopted is three-fold. In the first place it is to be preferred to the method of determining the position by the scale-divisions of the graduated dial, because it eliminates the random influence upon the attention of certain of the scale-divisions and concentrates the attention uniformly upon a single mark, which the index-hand is to reach simultaneously with the entrance of the disparate impression. Secondly, it takes precedence over the method whereby the subject selects the position in question upon an entirely

unmarked dial, for the reason that there is in this case no mark which may serve the attention as a fixation-point to which it may attach itself as an aid to the subject in determining his judgment, or upon which he may depend for reference in designating subsequently the selected position, without involving the element of memory. And lastly and perhaps of chief importance among the features essential to the method to which my apparatus is adapted is the elimination of eye-movements around or across the dial. For here the arrangement is precisely the opposite of that introduced by Klemm, and instead of an adjustable marker being brought to the position of apparent simultaneity of the impressions, the momentary stimulus is brought, as it were, to the marker or goal at the instant of the arrival there of the index-hand—the discrete stimulus being thus made simultaneous with the alignment of the index-hand and the goal. Thus the goal at which the disparate impressions are to be brought simultaneously is the focus of interest. Upon this the eyes are steadily fixated, instead of having to shift constantly to catch the position at which the stimulus enters, as in the method of Wundt and others.[1] Finally, my method shows a further departure from the routines adopted heretofore (with the exception of Klemm's) in that it presents the factor of two opposite directions of approach, *i. e.*, from definite positions on the side, where the stimulus clearly comes before the alignment and from definite positions on the side, where alignment precedes stimulus, thus neutralizing in the total of the results of the two types the influences due to direction of approach.

My apparatus was constructed from a Schumann time-machine[2] in the following way. A metal disc *M*, 25 cm. in radius, bored through the center to fit the shaft *S* of the time-machine, was slipped over the latter and rested horizontally upon the pulley *P*. Resting upon the metal disc was a corre-

[1] While the apparatus employed by me is similar to Klemm's in the introduction of the method of self-fixation, it was devised entirely independently. My experiments had continued for some months prior to the appearance of Klemm's article, in which is presented a description of his device.

[2] *Zeitschr. f. Psychol.*, Bd. 17. S. 253. See Catalog of Spindler & Hoyer for cut.

sponding disc of white card-board *D*. Both discs rotated with the shaft and with the contact arm *T*, but the position of the card-board disc and therefore of the index-hand could easily be changed. Upon the card-board disc was pasted the index-hand *H*, which consisted of a black strip, 11 cm. in length, lying upon a radius and terminating at the edge of the disc. The strip was wedge-shaped, its apex at the edge of the disc being 2 mm. in width, and its base 14 mm. In the horizontal plane with the card-board disc was a card-board dial, presenting an originally rectangular surface, in one edge of which, equidistant from either end, was niched a semi-circle having a radius slightly greater than that of the disc and placed concentrically and nearly touching the disc. The disc therefore rotated in close proximity to the concave edge. From the center of this concave edge, a scale was drawn outward 75° toward either side, the scale-divisions, drawn lightly with pencil at 5° intervals, being only with difficulty visible to the observer. At the mid-point for a mark or goal *G* was pasted a strip of black paper similar to the index-hand, but having a length of only 6 cm., and width tapering from 1 cm. to 3 mm. This strip was placed radially, its narrowest end at the edge of the card-board.

The circular brass scale *N*, beneath the revolving parts of the time-machine was cleared of all impedimenta save the single contact-block which moves with the lever *L*. A mercury contact *R Q* was employed when working with a light-stimulus, and a metallic contact in the case of the sound-work.

A small Zimmerman sound-hammer placed directly beneath the center of the dial served for the auditory-stimulus. For the light-stimulus I used the flash of a Geissler tube *E*, 12 cm. in length, placed horizontally about 12.5 cm. above the center of the dial and actuated by an induction coil. The resistance of the flash-circuit was so great that the current did not jump across at the make of the primary circuit. There was therefore only one flash, namely, that caused by the break of the primary circuit, A condenser of sufficient capacity, in parallel with the break, prevented any disturbing sparking

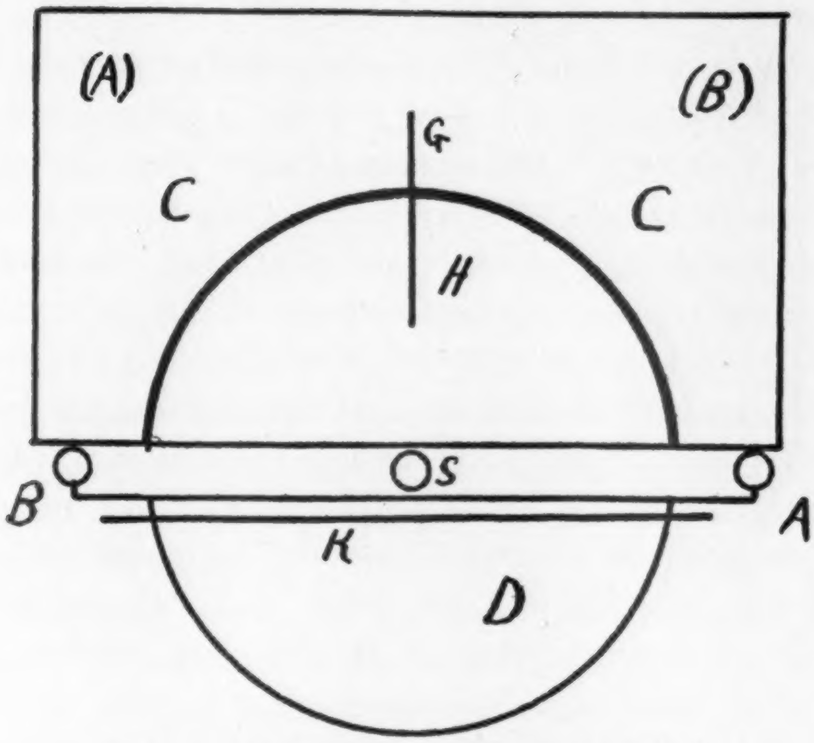

Fig. 1. Horizontal plan of disc and dial.

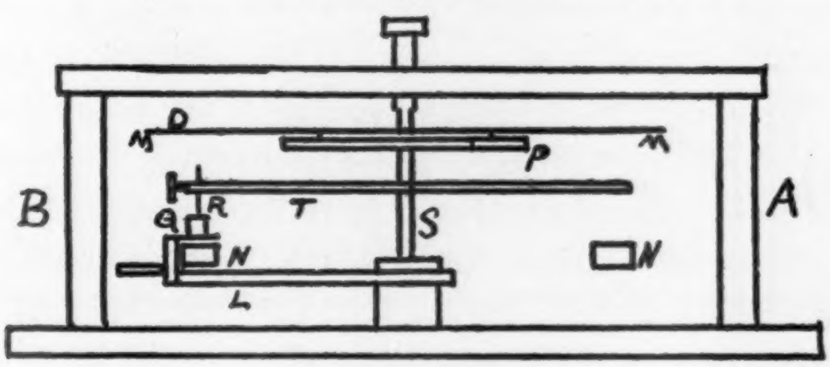

Fig. 2. Cross-section.

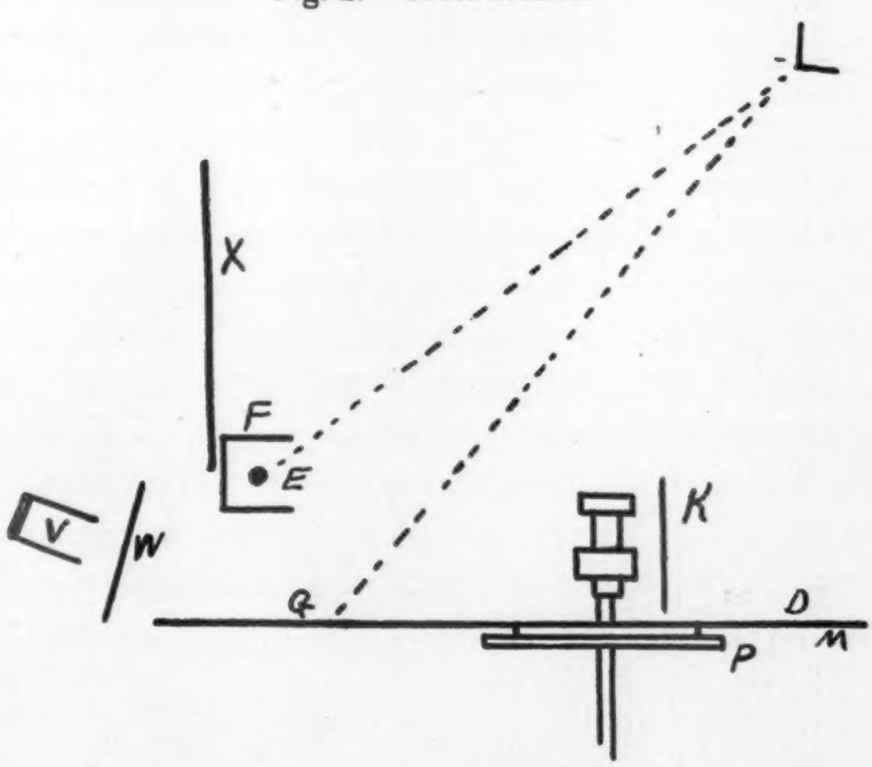

Fig. 3. Cross-section.

at this point. The flash in the Geissler tube was entirely inaudible to the subject. The tube was suspended in an oblong card-board box *F* (20 × 7.5 × 7.5 cm.), the anterior side of which was wanting. The interior surface of the box was black. The box was used in order to secure a more vivid flash, by intercepting the background of light which illumined the field of operation, and also to prevent the light from falling upon the dial and disc; which would have made an instantaneous photograph on the retina of the position of the index-hand. A screen *X* of black card-board above the box containing the Geissler tube shielded the eyes of the observer from the incandescent lamp *V* serving as constant source of illumination, The light of the latter was diffused by means of a plate of ground glass *W* placed vertically in front of it.

The machine was rotated by a belt transmission from an electric-motor which gathered impetus very quickly and maintained speeds of great uniformity, which were accurately controllable by means of a rheostat. The four rates I selected were one revolution in 2.25, 1.75, 1.25, and 0.75 seconds. The rates were obtained by observing the second-hand of a watch, until, through appropriate adjustment of the strength of the current in the rheostat there were four flashes (or clicks) in 9, 7, 5, and 3 seconds, according to the rate employed, the counting being continued in any one observation over five or six times these intervals. A key in the primary circuit enabled the experimenter to omit the flash or click at will.

The table upon which the time-machine rested was of such a height that the white disc was about upon a level with the eyes of the observer when seated. A screen *K*, 6 cm. in height and of the length of the diameter of the disc, placed just back of the center of the latter, intercepted the observer's view in the intervals of experiment. The long axis of the machine extending between the two vertical brass pillars was at right angles to the direction in which the observer faced, hence the finger lever could be moved in a clear sweep from pillar to pillar or through an arc of about 150°. For convenience of

designation, the side of the circular brass dial at the right of the observer was called the 'A' side, and so denoted by the letter *A* placed at the right-hand end of the path, and correspondingly the side of the brass dial at the observer's left hand was called the 'B' side and marked accordingly. Correspondingly for the experimenter who stood opposite the observer the quadrants on the white card-board scale on his right and left were labeled the 'A' and 'B' quadrants respectively.

The method of procedure was as follows: the room was darkened except for the incandescent lamp serving to illumine the dial and the disc over its anterior half. The experimenter called 'fix "A"' (or 'fix "B"'), whereupon the observer moved the finger-lever to the *A* (or *B*) side, in the neighborhood of the right (or left) column, and rotated the contact-arm *T* until, in the case of the auditory stimulus, the metallic strip attached to the arm made contact with the center of the surface of the metallic block, attached to the finger-lever; or, in the case of the visual stimulus, until the needle *R* stood at the middle of the substituted cup *Q*, (the circuit during this procedure being of course interrupted by the key as above described). The revolving arm being held in this position, the experimenter shifted the card-board disc until the index-hand was brought to the desired position, as indicated by the numbered scale-divisions in the quadrant of the dial diagonally opposite to the position of the lever—in the 'A' quadrant if the lever is at the 'A' side, and in the 'B' quadrant if the lever is at the 'B' side. The experimenter then said 'Ready,' at which signal the observer released the contact-arm, and the experimenter closed the switch that started the motor, setting the machine in motion. When a uniform speed had been attained, for which three or four revolutions of the disc sufficed, the experimenter said 'now' and closed the key controlling the discrete stimulus. At 'now' the subject rose and with hand on finger-lever, fixating the goal, observed (by indirect vision) whether the disparate impression occurred synchronously with the passage of the index-hand, at a position within the 'A' quadrant, or at a position in the 'B' quadrant, *i. e.*, whether the complicating stimulus occurred at a position in front of the goal,

relative to the direction of rotation of the disc, as in the case of 'A' settings, or at a position behind the goal as in the case of 'B' settings. Then, still steadilv fixating the central or zero position indicated by the goal, he moved the finger-lever and hence the position of the discrete stimulus in the path of the index-hand until, by advancing or retracting it, as necessary, the stimulus appeared simultaneous with the alignment of the index-hand and the goal. When satisfied that the object had been attained, the observer so indicated, and took his seat, whereupon the experimenter opened the switch controlling the stimulus-circuit and the switch controlling the motor. At the word 'set' the observer without moving the finger-lever rotated the arm until contact was made as before, enabling the experimenter to take the reading by the index-hand which was thus set at the position of judgment. The position being recorded, the procedure was repeated as before.

The technique specifically adopted called for the alternate shifting of the finger-lever, and hence of the stimulus, from 'A' to 'B.' Beginning each experiment-period with 'A,' six successive judgments were made with each rate, that is three upon the 'A' side and three upon the 'B' side, and with a uniformity in the order of the positions at which the preliminary setting was made in each trial. From side to side the positions were (in degrees)

A	B
60°	45°
75°	60°
45°	75°

Thus was obtained a perfectly definite order of starting points, which yet seemed to the subject hap-hazard as regards their distance from the goal. The above order was selected with a view to securing the utmost possible balance between the two sides.

Further, this provision for irregularly alternating succession of positions upon the 'A' and 'B' sides was made, along with the instruction to shift the lever 'carelessly' *i. e.*, avoid-

ing returning it always to exactly the same position on either side, as a precaution against prejudicing the observer's results through his discovering any rigid method of setting out, and so inspiring in him an anticipatory feeling of the length of arc to be traversed by the discrete stimulus in order to bring it to the goal simultaneously with the index-hand.

With regard to the designation of the direction of error I follow precedent of Wundt and others, by designating as positive those errors in which the assumed position is later than the real position; and as negative, the errors in which the assumed position is earlier than the real one. Wundt read therefore from actual to assumed position in every case. Accordingly if the index-position which the subject judged to be at zero was really in the 'A' quadrant, as shown in the reading from the 'set' position, the error was positive; if the index-position which the subject judged to be at zero was really in the 'B' quadrant, the error was negative.

With regard to the order of the succession of the velocities: this was taken in three ways. In the first I began with the fastest rate and proceeded to the slowest; in the second I proceeded in the reverse direction, beginning with the slowest and gradually going over to the fastest; and thirdly the succession of rates was arranged in such a way that the rates intermediate in speed were made the extremes in the order of succession, and *vice versa*, so that the series was now 1.25, 0.75, 2.25, and 1.75.

With the light stimulus it was necessary to make a correction for the lag of the stimulus, due to the drag of the mercury in the cup, *plus* the latency of the spark-coil and Geissler tube, which together amounted, with the different rates, from fastest to slowest, to 2σ, 6σ, 8σ, and 14σ, respectively, to be subtracted from the results. This correction was obtained by turning out the light which normally illumined the dial, and inverting the box containing the Geissler tube, so that the tube was hidden from the eye, and the flash illuminated the dial and the index-hand. The index-hand was set by the usual method to read zero at the moment of the flash, and then put in motion at the speed for which the error was to be determined. In

this way a photographic image of the actual position of the index-hand at the moment of the flash was produced on the retina and the deviation from the zero point could be readily noted. Black dots placed upon the scale-divisions facilitated the reading of the actual position due to the lag. The lag of the Geissler tube and spark-coil was independently determined by physical methods and found to be never greater than 1σ. The greater part of the lag was therefore due to the mercury-cup itself. No method is available for determining the latency of the physiological process in the retina.

Likewise to allow for the lag of the hammer employed for the sound stimulus, it was necessary to subtract 6σ, 5σ, 3σ and 2σ from the results obtained with the rates from fastest to slowest, respectively. These lags were accurately determined from kymographic records of the movement of the hammer and a tuning-fork curve, together with the time elapsing at each rate between the first making of contact and the instant of reaching the central point on the contact-block at which the readings were taken throughout the experiment. Further, account was taken of the time required for the sound from the hammer to reach the ear. As the hammer was approximately three feet from the subject's ear, this was assumed to be 3σ.

The corrected results of the first portion of my experimental work are given in part in Tables 1 and 2. Table 1 gives the average positions, *i. e.*, the averages of the displacements of the judged positions for each individual subject and rate for the sides 'A' and 'B' and for 'A' and 'B' taken together. The first column under 'subject' gives the five subjects by number. The second column 'rate' gives the seconds per rotation of the rates to which the figures correspond. The third column 'A' gives the average displacements when moving from the 'A' side, *i. e.*, when the flash was, at the beginning of the single experiment, much too early. The fourth column 'B' gives the average displacements, when moving the flash from the reverse or 'B' side. The fifth column 'total' gives the averages of the figures in the 'A' and 'B' columns. The sixth, seventh, and eighth columns give for the experiments

with sound the averages corresponding to those in the third, fourth, and fifth columns for light.

Table 2 gives the average variations corresponding to the averages in the 'A' and 'B' columns of Table 1. No average variations are given corresponding to the 'total' columns in Table 1, because the only significant figures for such purpose

TABLE I.

Subj.	Rate	LIGHT.			SOUND.		
		A	B	Total	A	B	Total
I.	0.75	+ 0.4	−21.0	−10.3	−23.3	−22.9	−23.1
	1.25	−14.0	−41.2	−27.6	−46.0	−51.2	−48.6
	1.75	−10.4	−49.3	−29.8	−36.2	−45.1	−40.6
	2.25	− 4.7	−40.0	−22.3	−23.8	−38.4	−31.1
II.	0.75	+ 9.4	+22.6	+16.0	− 0.2	+15.1	+ 7.4
	1.25	− 2.0	+ 2.6	+ 0.3	− 2.2	+ 3.6	+ 0.7
	1.75	−23.3	−18.5	−20.9	+ 3.4	+ 9.1	+ 6.2
	2.25	−18.1	−10.9	−14.5	+ 3.2	+11.5	+ 7.3
III.	0.75	+16.0	−17.9	− 0.9	+22.0	+12.3	+17.1
	1.25	+26.9	+ 0.3	+13.6	+32.5	+27.9	+30.2
	1.75	+24.4	+ 0.9	+12.6	+37.5	+37.5	+37.5
	2.25	+16.2	− 6.8	+ 4.7	+33.4	+33.4	+33.4
IV.	0.75	− 5.1	+ 0.4	− 2.3	−33.7	−40.3	−37.0
	1.25	+ 3.8	− 7.1	− 1.6	−28.7	−41.4	−35.0
	1.75	+ 4.1	+ 9.0	+ 6.5	−30.3	−29.5	−29.9
	2.25	+ 2.6	+ 0.5	+ 1.5	−35.3	−32.2	−33.7
V.	0.75	−31.4	−37.7	−34.5	−15.3	−24.0	−19.6
	1.25	−44.7	−62.6	−53.6	−45.4	−61.0	−53.2
	1.75	−28.2	−25.0	−26.7	−53.2	−37.8	−45.5
	2.25	−40.0	−43.1	−41.5	−68.6	−49.9	−59.2

would be the average of the variations for 'A' and 'B', the 'A' and 'B' averages in Table 1 forming distinct groups with variations about independent centers of equilibrium, as will be more fully brought out. If the average variation from the total were taken in the ordinary way, it would simply be adding the variable error to the constant error which is an entirely

unwarranted proceeding. The variations are given in their absolute values, not relative to the average displacement, because the measure of the average displacement is simply a convenient way of denoting a position on an arc of a circle by its distance from an arbitrarily fixed point. Other things

TABLE 2.

Average Variations.

Subj.	Rates	LIGHT		SOUND	
		A	B	A	B
I.	0.75	28.4	21.4	16.9	19.5
	1.25	23.1	22.4	10.9	16.7
	1.75	23.4	16.2	10.5	13.7
	2.25	16.6	19.7	14.5	17.7
II.	0.75	16.9	16.9	10.0	12.8
	1.25	12.7	15.0	8.9	8.6
	1.75	7.2	15.3	7.2	8.1
	2.25	19.7	13.5	8.3	7.2
III.	0.75	15.6	21.4	19.0	12.8
	1.25	18.5	15.6	12.1	17.9
	1.75	20.2	16.2	11.3	17.0
	2.25	15.6	25.0	13.5	17.7
IV.	0.75	16.9	16.9	8.6	14.2
	1.25	13.3	16.1	9.2	11.5
	1.75	20.2	13.7	6.4	10.5
	2.25	15.6	17.7	10.4	8.3
V.	0.75	27.3	31.8	22.5	23.2
	1.25	25.4	27.1	19.0	31.8
	1.75	32.4	33.2	18.6	33.2
	2.25	30.2	27.8	26.0	29.1

being equal, we should have as much reason to expect a variable error of a given magnitude at one point on the arc as at another. It is obvious that if the average variation were expressed in percentages or in some such way in relation to the magnitude of the displacement, a displacement of zero (which might readily occur, as seen by the value of 0.4 in two cases) would

necessarily make the number representing the variable error infinitely large, which furnishes the *reductio ad absurdum* for the proportional statement of variable error and constant error. All values are expressed in σ throughout. Other experimenters fail to include in their results a table of average variations. The omissions are unfortunate, for a comparison of the average variations of my subjects with those of the subjects of the other experimenters would be interesting.

Five persons served as subjects for my investigation: Prof. G. M. Stratton, Dr. Knight Dunlap, Dr. W. D. Furry, Mr. T. A. Lewis, and myself. The experiments here reported were conducted for a period of two years, from the spring of 1907 to the spring of 1909.

My tables are constructed on the basis of the averages of 72 judgments for each subject, with each of the four rates employed, of which 36 are judgments made in the 'A' quadrant and 36 made in the 'B' quadrant.

First to be remarked is the fact that displacements, both positive and negative, occur when a momentary *light* impression is substituted for the discrete auditory stimulus hitherto employed in the complication experiment. In general the displacement with the visual stimulus is numerically less than with the auditory. The only marked exception is the case of Subject II, whose errors for each rate but one are greater with the light than with the sound impression. It is further seen that in general the direction of the displacement is the same for a given subject with both forms of stimuli, the exception being Subject IV, whose errors are all negative with the sound-stimulus, while with the light-stimulus his positive errors predominate. It is noteworthy that for each subject, with the sound, the direction of the displacement is consistently the same for each of the eight groups of judgments, (the exception appearing in the case of Subject II, whose displacement is negative in direction for the 0.75 and 1.25 sec. rates in the 'A' quadrant, all the other averages being positive) while for light there is among the groups corresponding to different rates considerable discrepancy as to the quality of the error.

With regard to the effect of the rate of rotation of the index-

hand, and consequent rapidity of the succession of the discrete impressions: the relative displacement for the four rates employed differs with different subjects in amount and direction. For example, with Subjects I and II, the tendency is toward a more negative displacement with the slower rates, which accords with the results of Wundt and the other German investigators, while on the other hand, in the case of Subjects III and IV, a directly opposite tendency is shown, the lower rates producing a more positive result. With Subject V, no uniformity is to be traced in this respect. Further, the relative magnitude of the displacement for different rates, disregarding now the sign, varies with different subjects. For example, with the light-stimulus, the magnitude of the errors of Subject I are least with the 0.75 sec. rate, while the least error with Subject IV is at the 2.25 sec. rate, and for Subject V, at the 1.75 rate.

Considering now the results of 'A' and 'B', a comparison of the error obtained in each pair in the case of Subjects I, III, and V shows that the tendency of judgments, made when the position of starting out is in the 'A' quadrant, is positive in relation to 'B', that is to say that, in moving from 'A' or 'B', the tendency is to stop on the corresponding side of the average position of the two.

Comparing the errors of 'A' and 'B' in the case of Subject II, gives a result which is just the reverse of that given in the three cases cited. This discrepancy is only apparent however and the explanation is to be sought in the reversed procedure, which is characteristic of this subject's method in making a determination and in accordance with which the 'A' judgments and the 'B' judgments became virtually transposed. For in every judgment it was the practice of this subject to continue shifting the temporal position of the discrete stimulus from the starting position either before or after the alignment of index-hand and goal, until he found that he had overstepped the position of simultaneity, and hence was obliged to return to it. So that in setting out from the 'A' side, let us say, he invariably passed over to the 'B' side of the scale, until assured of a discrepancy, when he would finally set about making the

discrete stimulus synchronous with the alignment—hence a 'B' judgment. In setting out from the 'B' side, his procedure was similar; the ultimate judgment becoming in reality an 'A' judgment. This explanation was not elicited from the subject after noticing the variance of his results. His manner of procedure had caught my attention from the first, without my seeing in it at the time any ulterior significance. Later, the subject himself, during the course of an experiment, voluntarily called attention to the fact that his procedure was invariably the reverse of the normal method above described. The subject was of course entirely in ignorance of his results when he made this statement. In the light of this explanation, the results which Subject II show with regard to the 'A' and 'B' direction of approach are entirely consistent with the results of the other subjects, so that the tendency of the 'A' judgment to stop on the 'A' or positive side of the average position of 'A' and 'B', and of the 'B' judgments to stop on the 'B' or negative side of the average position of the two is characteristic in general.

It may be noted that with Subjects I and III, the interval between the values of 'A' and 'B', that is, the distance of 'A' and 'B' from the average of both is much greater in the case of the Light impression than for Sound. For Subject II, the influence of the new position of setting-out (that is, reversing the direction after having crossed the goal) cannot but be off-set in a measure by the counter-effect of the influence of the original starting-point. Thus the effects of the two influences are composed in the results for Subject II, and probably account for the small separation of the 'A' and 'B' averages in his case. Subject IV presents results also less regular in this regard, particularly where the stimulus used was the light, but the explanation turns upon the same point as in the case of II, it having been also (though not invariably) the practice of this subject to over-reach the goal and then return to it.

An important factor which my experiments bring to light is the influence of the position of setting out upon the direction and magnitude of the final judgment. The effect is specifically shown by a comparison of the different results obtained,

when setting out from different determined positions upon either side of the goal. To test this influence I compared for each subject the averages of the twelve judgments, obtained with each rate with the different positions of origin; that is, when starting out from the position of 75°, 60°, and 45° on the 'A' side and when starting out from the corresponding positions on the 'B' side. This comparison was made in the case of both light and sound stimuli. The results are given in Tables 3 and 4, for light and sound respectively. The averages are here given in degrees. The tables show the

TABLE 3.

Light.

STARTING POSITIONS. IN DEGREES.

Subj.	Rates.	A.			B.		
		75°	60°	45°	75°	60°	45°
I.	0.75	+15.80	+ 3.10	−12.30	−18.4	−12.10	+ 1.5
	1.25	− 1.7	− 3.8	−13.1	−17.0	−11.9	− 3.9
	1.75	+ 1.7	+ 2.0	− 6.5	−11.3	− 6.4	− 5.0
	2.25	+ 2.5	+ 3.6	+ 1.4	− 5.4	− 3.8	− 2.1
II.	0.75	+11.83	+ 0.66	− 2.08	+ 8.58	+12.75	+14.08
	1.25	+ 0.33	+ 1.5	− 1.25	+ 5.0	+ 1.25	+ 0.16
	1.75	− 3.25	− 4.25	− 3.75	− 3.83	− 0.83	− 1.5
	2.25	− 0.41	+ 0.16	− 2.66	+ 0.25	− 0.83	+ 1.0
III.	0.75	+18.25	+10.66	+ 0.25	−18.08	− 8.91	− 0.16
	1.25	+12.91	+10.33	+ 2.66	− 1.5	+ 1.75	+ 5.48
	1.75	+11.5	+ 6.0	+ 1.75	− 0.75	+ 1.91	+ 5.16
	2.25	+ 5.58	+ 6.5	+ 2.91	− 1.34	+ 2.5	+ 2.16
IV.	0.75	+ 3.0	+ 2.16	− 6.25	− 7.33	− 0.33	+ 7.33
	1.25	+ 4.08	− 0.48	+ 3.91	− 0.58	+ 2.0	+ 0.58
	1.75	+ 4.83	+ 2.5	− 0.16	+ 2.91	+ 3.58	+ 5.5
	2.25	+ 4.5	+ 1.75	+ 0.91	+ 2.25	+ 4.75	+ 1.58
V.	0.75	− 1.33	−16.33	−28.25	−31.5	−23.66	− 5.0
	1.25	− 6.66	−12.25	−19.41	−22.16	−18.91	− 7.41
	1.75	+ 0.16	− 4.33	−10.25	− 7.41	− 4.5	+ 1.91
	2.25	− 2.33	− 3.16	− 6.58	− 6.66	− 8.0	+ 0.66

TABLE 4.

Sound.

STARTING POSITION. IN DEGREES.

Subj.	Rates.	A.			B.		
		75°	60°	45°	75°	60°	45°
I.	0.75	− 1.91	− 8.0	− 8.75	−18.41	− 7.08	− 2.83
	1.25	−11.08	−14.33	−12.25	−15.91	−12.41	−10.00
	1.75	− 5.08	− 5.16	− 7.83	− 9.91	− 7.41	− 7.75
	2.25	− 2.66	− 2.75	− 4.25	− 8.5	− 5.41	− 4.25
II.	0.75	+ 5.58	+ 5.41	+ 1.0	+ 5.5	+10.16	+18.25
	1.25	+ 3.33	+ 0.5	+ 1.33	+ 0.75	+ 3.83	+ 5.08
	1.75	+ 3.08	+ 1.16	+ 0.83	+ 1.91	+ 3.25	+ 4.25
	2.25	+ 3.16	+ 0.16	+ 0.33	+ 1.08	+ 2.66	+ 3.75
III.	0.75	+21.25	+15.25	+ 6.0	+ 2.83	+12.08	+14.66
	1.25	+14.41	+12.16	+10.25	+ 4.91	+11.0	+16.75
	1.75	+11.33	+ 9.0	+ 7.41	+ 6.08	+10.5	+12.5
	2.25	+ 8.83	+ 5.08	+ 5.5	+ 3.58	+ 7.0	+ 8.91
IV.	0.75	−13.66	−13.91	−16.5	−23.58	−17.08	−12.58
	1.25	− 6.41	− 5.41	−10.91	−10.66	−10.91	− 8.58
	1.75	− 6.0	− 5.33	− 6.83	− 7.33	− 7.0	− 7.08
	2.25	− 4.91	− 6.16	− 4.91	− 6.58	− 3.83	− 5.91
V.	0.75	+ 0.75	− 2.66	−19.83	−18.58	−10.5	− 2.08
	1.25	− 9.33	−10.83	−18.75	−28.0	−11.75	− 6.66
	1.75	− 9.41	− 7.0	−14.5	−12.25	− 8.33	− 1.08
	2.25	−10.16	− 9.0	−12.5	− 9.83	− 7.41	− 4.08

remarkable regularity of the influence of the starting point in the coincident progression of final judgments and original positions, the final judgments of the 'B' side becoming progressively more positive as the starting point is shifted in a positive direction and correspondingly the final judgments of the 'A' side assuming a more negative position parallel with the more negative shift of the initial setting of the index-hand.

Because of the resulting feebler contact as the rates became progressively faster, the intensity of the stimulus diminished with increasing rates. As this factor introduced the question whether the varying results obtained with different rates were

due to the corresponding variation in the intensity of the stimulus, new experiments were undertaken with a view to studying the influence of the intensity of the discrete stimulus. Using Subjects I, II, III and V, I obtained with each on three different days, eighteen judgments with a weak intensity, and the same number with a stronger intensity, the eighteen judgments consisting in both cases of nine of 'A' and nine of 'B.' Sound was used throughout this variation of my experiment. I proceeded by giving groups of six trials, that is, three each of 'A' and 'B,' and alternating the intensity with successive groups. The two intensities were designated 'Weak' and 'Strong,' the weak being the weakest stroke employed in the experiments, and the strong consisting of a very loud, sharp stroke. The results as shown in Table 5 were practically negative, the only considerable difference occurring in the averages for Subject V, whose determinations were always erratic. The values in Table 5 are given in σ, and are the excess of the average errors with 'Strong' stimulus over the average error with 'Weak.'

TABLE 5.

Intensity. Sound. Rate 1.25.

Strong *minus* Weak.

Subj.	A.	B.	Total.
I.	− 3.47	+ 3.47	0.0
II.	+ 0.01	+ 3.47	+1.74
III.	0.0	0.0	0.0
V.	−10.41	+17.35	+3.47

Since along with Wundt's denial of the possibility of obtaining a displacement in cases in which two momentary stimuli are of the same sense, he excepts the cases in which the conditions make possible the wide separation of the two stimuli, as upon the upper extremity and the lower extremity, or with one and another of a pair of organs of the same sense, as with first one eye and then the other; a series of experiments were now made in which the two visual impressions, the flash and the passing of the index-hand, fell upon contiguous areas of

the retina. The reader will recall that while, in the first experiments, the impressions were on adjacent portions of the retina, they were not strictly contiguous, the Geissler tube being separated from the goal (and its neighboring regions) by an interval of approximately 12.5 cms. The condition of actual contiguity was satisfied by inverting the box containing the Geissler tube, so that the flash fell upon the dial, while the subject did not see the tube at all; the margin of the area illuminated by the flash coinciding with the edge of the dial and hence of the scale, along which moved the index-hand. The flash was limited to the dial in this manner, and not permitted to fall upon the index-hand, for in such a case a retinal photographic image of the actual place of the index-hand would have been produced, thus precluding the conditions essential to the conduct of the experiment. The results of this experiment are given in Table 6. Each number given under 'A' and 'B' in this table expresses in σ the average of twenty-four judgments. As the results show, where conditions are introduced which cause contiguous visual images to fall upon the retina, a time-displacement occurs not differing in direction from the time-displacement observed where the stimuli were allowed to fall upon nerve-terminals of the retina which were more remote from each other.

TABLE 6.

Contiguous Visual Stimuli. Rate 1.25.

Subj.	A.	B.	Total.
I.	−10.05	− 42.5	−26.1
III.	+49.04	+ 23.0	+36.02
IV.	− 9.85	− 32.6	−21.22
V.	− 7.45	−103.05	−55.25

In the case of Subject I, the magnitude of displacement is about the same in the two cases. In the cases of III and IV the magnitude is greater with the contiguous stimulation; and in the case of V, although the magnitude of the total is about the same, 'A' and 'B' are farther apart.

To test the validity of the alternative requirement which Wundt makes as a condition of obtaining a temporal displacement in the case of two simultaneous impressions belonging to the same sense-realms, namely, that they occur each upon one of a pair of organs of the same sense, I conducted a few experiments in which the subject observed with monocular vision.

Performing a series of experiments upon different days on Subjects I and IV, and having them alternately use the right and left eye, I again obtained results, agreeing essentially with the readings obtained when the subjects presumably employed binocular vision, but showing a more exaggerated displacement. If, however, it be suggested that even when both eyes were open, the subjects really employed monocular vision, the results are in no wise affected.

The general method of my experiments, *i. e*, the active approximation, by the subject, of the discrete stimulus to a fixed position of the index-hand, introduces, as has been clearly brought out above, factors which are not explicitly present in the judgments obtained by the older methods, although it is by no means certain that they are not operative in those cases. In order to find whether the general averages, in which these factors may be supposed to compensatefor each other, differ much from results obtained by the older methods, further experiments were undertaken with Subjects I, III, and V, in accordance with the Wundt procedure. For this purpose another scale was made which did not bear the black strip at the zero point to indicate the goal, but bore instead the scale drawn in black lines 13.5 mm. long, and 1 mm. wide, five degrees apart, and extending 75° in both directions from the central point. For accuracy in recording, these were numbered in pencil, but so faintly as to be indiscernible to the subject. Following the classic methods the experimenter placed the discrete stimulus at a position of the index-hand unknown to the observer, and when the latter had declared the position at which the index-hand seemed to be, at the moment of the discrete stimulus, the two positions were compared and the difference between them, *i. e.*, the magnitude of the displace-

ment, if any, recorded; the quality of the displacement as usual being called positive if assumed position comes *after* the real one, and negative if the apparent position comes *before* the actual position.

In these tests, the series used, in the experimental hour, consisted of nineteen different 'settings' of which nine were at different positions in the 'A' quadrant and nine at different positions in the 'B' quadrant, the other position being at zero. Only one rate was used, the 1.25. The discrete stimulus was necessarily sound. There was but one series given in the case of Subject I, and for Subjects III and V, two series each were obtained. The results are given in σ in Table 7, each number under (A) and (B) being the average of nine determinations, namely, the nine given in the quadrants designated as A and B respectively in the previous experiments.

TABLE 7.

Passive Method. Rate 1.25.

Subj.	(A)	(B)	Av.
I.	+20.82	− 41.64	−10.82
III. (1)	+34.7	− 27.76	+ 3.47
(2)	+38.17	− 48.58	− 5.20
V. (1)	−45.11	− 90.22	−67.67
(2)	−65.93	−100.63	−83.28

For Subject I, the displacements for the settings in the 'A' quadrant were all positive, and in the 'B' quadrant all were negative. The same was true for Subject III, with three exceptions in the 'A' quadrant, although the average was there positive. Subject V is so far in agreement with the other two that the displacements are more negative for the B quadrant.

III. DISCUSSION AND INTERPRETATION.

The experiments which I conducted after the method of Wundt (see Table 7) brought out a result strikingly different from what was found by Wundt and Geiger. This result was the displacement of the apparent simultaneity in opposite direc-

tions on the two halves of the dial, the errors being almost all positive in the 'A' quadrant and negative in the 'B' quadrant. It is true that the German experimenters had found a difference in this direction between the results for the two halves of the dial, explained by them as due to eye-movements; but the difference they found was merely a greater positivity (or less negativity) on the one side as compared with the other, and not a complete reversal with considerable magnitudes.

Now this opposition between the results given in the 'A' and 'B' quadrants is obviously the outcome of some feature of my experiment, which is not present in the experiments of Wundt and others. It will be remembered that in this part of my work the dial presented to the subject was uniformly scaled throughout, there being no conspicuous mark to indicate the middle point of the dial, as was the case in my other experiments.

This difference between the errors in the two quadrants agrees in kind with the difference between the errors when moving from 'A,' and the errors when moving from 'B' in the preceding experiments on the same observers, although in these preceding experiments the factor of eye-movement is either excluded or at least reduced to minimal terms. It is therefore impossible to escape the conclusion that the factor which produced the results in question in my experiments after the Wundtian method was carried over from the preceding experiments; for, furthermore, the conditions of my last experiments differed from those of Wundt and Geiger essentially in being preceded by these other experiments by a different method.

The factor in question probably consists in the greater attention to the middle point of the scale, resulting from the preceding experiments in which the middle point indicated by the black strip ('goal'), was intentionally made the focus of interest. It is impossible to suppose that the habit of fixing the attention upon the middle position, to the nurture of which the usage of the preceding experiments with these subjects contributed, is not unconsciously carried over into the subsequent experiments, with the effect of influencing the judgments in a manner

similar to that in which the preceding judgments were influenced.

Geiger noted a factor analogous to the one in question, which consisted in the fortuitous effect upon the attention of the uppermost and lowermost positions of the scale. This factor first appeared in the experiments in which the telescope was interposed, thus introducing a division of the dial into quadrants by the cross-threads of the instrument. But as Geiger argues, an influence analogous to this actual partitioning of the dial by means of the telescopic field, is doubtless operative, though of course in a less degree, in experiments of the original form. For there too, the top and the bottom at least of the dial become, as it were, critical points in the path of the index-hand. The very fact of these points being uppermost and lowermost gives to them of itself a feature of distinction. Further, these points represent for us, as they do in all symmetrical objects, median positions. These points possess then of themselves a certain specific interest, analogous to the interest we find in general in the median region of all bilaterally symmetrical objects, as especially exemplified in the human body.

So much for the influence of the 'Umkehrpunkt,' as brought out by Geiger, upon determinations made with the complication-clock. What of its influence upon determinations made in experiments in which the complication-pendulum has been the apparatus employed? From an analysis of the different elements entering into the conditions of experimentation with the pendulum apparatus, we may justly infer that the effect of a critical point is still more potent in the use of this machine, because of the oscillation of the index-hand about a middle point. For in this form of apparatus we have in addition to the effect upon the attention of a middle point of the scale, as cited by Geiger, likewise the influence due to the fact that here the middle point on the scale is also the middle point as regards the path over which the index-hand swings. This latter factor has the effect of emphasizing the middle position in a two-fold manner, that is, in regard to space and in regard to time; for the length of vibration, measured in degrees, of the index-hand

upon one side of the mid-position is exactly equal to the length of its vibration upon the other, and the time of vibration of the index-hand, upon one side of the central point is exactly equal to the time of vibration upon the other. Hence it is the central point, temporally as well as spatially considered. Finally, a further factor tending to give preëminence to this middle position lies in the fact that this position is the only one throughout the entire excursion of the index-hand at which the velocity is constant. Toward this point and only as far as up to this point, the index-hand is increasing in velocity; leaving this point and immediately after passing it the velocity is decreasing, so that independently of the influence upon the error of the factor of acceleration and retardation *per se* (if any such influence exist), this factor tends indirectly to emphasize in a special manner the position at which the index-hand is at its center of oscillation; and thus is added a third element to the general influence serving to emphasize the attraction of the middle or uppermost point upon the scale. Of course this last element, as well as the other elements contributing to the total influence tending to render prominent the critical, turning-point in the path of the index-hand, is throughout unconsciously operative. The question that here suggests itself is whether, after all, the difference in the results on the two sides of the dial, where the pendulum machine was used, was really due to the factor of acceleration and retardation, or rather to this factor of the difference in the influence of the central point in the scale, according as the position of the index-hand was on the one or the other side of it when the bell-stroke or other discrete stimulus occurred.

Naturally the consideration of this factor calls into question the results of the work of Klemm, which, as above stated, was planned with a view to isolating the influence of the factor of eye-movements, according as the judged position was upon the side of ascent of the index-hand or upon the side of descent, from the influence of the factor of acceleration and of retardaation occurring concomitantly under the same conditions. Was the factor which Klemm thought he isolated really the factor of acceleration and retardation, or was it the factor which

our experiments have shown to be so considerable under the simpler conditions of experiment obtaining in the use of the complication-clock, and which, it may be justly inferred, would operate so much more effectually with the use of the pendulum apparatus—namely, the factor of the influence upon the attention of the chief or central point in the path of the index-hand?

We have, then, throughout all such experiments, the tendency to elevate in importance the uppermost or, in Klemm's work, the lowermost, region of the dial; but besides this unconscious influence, it is the deliberate intention of my principal experiments to concentrate the interest upon this region, and make of it the goal to be attained by the index-hand at the moment of the discrete impression.

Let us call this factor of the attraction of the predominant point of interest, which is seen to be especially operative in my experiments, factor No. 1. Now, assuming this factor alone to be present, judgments from the 'A' positions, in the normal procedure, in which the subject moves the position of the stimulus directly toward alignment of the index-hand with the goal, would be invariably on the 'A' or positive side,[1] and conversely, judgments from the 'B' positions would be always on the 'B' or negative side. For the effect of the attraction of the objective point or goal is to cause a given position of the index-hand to seem nearer the goal than it really is, and accordingly an actual position, which is a certain distance from the objective point will seem to be at that point. Positions still nearer the goal than that which the subject first judges to be the correct one may also seem correct, as shown in the case of Subject II, who actually moved to such nearer positions and even to a position beyond apparent simultaneity.

Now since results by my original method for each subject in nearly every rate show the determinations, when setting out both from the 'A' side and the 'B' side, to have the same sign, *i. e.*, to be on the same side of the goal, it is evident that

[1] Speaking of course in terms of my experiment, where the judged position was finally zero in each case, and the record of the judgment, the distance of the real position from that judged position.

a second cause or group of causes is operative which is sufficient to overcome the influence of factor No. 1, on at least one side of the goal. Since factor No. 1 changes the direction of its influence when the goal is past, or at least since it becomes a permissive influence in the new direction, the two factors are now really coöperating, *i. e.*, factor No. 1 now probably operates to permit an effect of factor No. 2 greater than it would have if factor No. 1 were not present. Certainly it does not lessen it. For example, Subject I in Table 1, gives throughout (with the single exception of the 'A', 0.75 rate, for light) negative determinations; which means that the determinations both from positions of setting out in the 'A' quadrant and from those of the 'B' quadrant were in the 'B' quadrant. Hence, in obtaining the former determinations the goal must have been crossed, which means that in addition to factor No. 1, which allows the determination to stop before reaching the goal, there was present a further influence which operates to carry the judgment by the goal and over to the 'B' quadrant or negative side.

A glance at Table 1 shows that while the movements from both directions terminate in the same quadrant, they do not terminate even approximately at the same point. From this it is clear that the terminus of the judgment in so far as concerns the influences as yet considered, is not a point but a region within which any point might serve. The subject therefore who proceeds to the position of his final judgment as in the normal way, moves into this zone and stops, whereas the subject who inverts this procedure moves through this zone, and then back into it. We may say then that the difference between the values of 'A' and 'B' represents the width of this indifference zone. What is here called the indifference zone, corresponds to the region to which Klemm applied the name of 'Simultaneitätsbereich.' What the constant factor (No. 2) is, which tends to give the average of 'A' and 'B' a negative or a positive value as the case may be, is a question with which we shall later concern ourselves.

The next factor to be considered is the movement of the position of the discrete stimulus from the starting point towards

the goal. This we may call factor No. 3. Whatever effect this factor may have, we should expect to be dependent upon the length of the movement, *i. e.*, whether startingout from a position at 75°, 60°, or 45° from the goal in one or the other quadrant. It is important to note that since the positions in question correspond on the two sides of the dial, and the movement is in opposite directions on the two sides, this influence must cancel on the averaging together of the 'A' and 'B' results.

Tables 3 and 4 show for the light-stimulus and the sound-stimulus respectively a pronounced effect of the different positions of setting-out on the two sides, the tendency being on the 'A' side to make the judgment more positive, when starting from a more positive position *i. e.*, from a position more remote from the goal; and conversely on the 'B' side, the judgments are more negative according as the position of commencing is more negative, *i. e.*, more remote from the goal. So that, in general, in commencing successively with positions of 75°, 60° and 45° on the 'A' side, the judgments become progressively less positive, while employing successively these same positions of starting out on the 'B' side, the resulting judgments are progressively less negative. This may be interpreted first, as an instance of the prevailing tendency to bring movements and even judgments to an average; or second, it may be regarded as due to an influence of the starting-position on factor No. 1 or on factor No. 2. The exact details of this second interpretation are not apparent while the first is probably true in any event. This influence of the starting-point whether expressed through the averaging of movements or other processes, or through the influence of the starting-point on the attention, and thus on factor No. 1 or No. 2, or on both, contributed largely to the variable error. If each group from a given starting-point is treated separately its variable error is low.

It was stated in the beginning of this section that the deviation in opposite directions of the judgments in the opposite ('A' and 'B') quadrants of the dial, as shown in the experiments I conducted after the manner of Wundt, might be ascribed

to the contrary effects of eye-movement upward in one quadrant and downwards in the other, except that under the conditions of my method of experiment the factor of following eye-movements is practically *nil*. I wish now to show why, in consideration of my altered method of experiment, this hypothesis is not to be entertained. It was the experience of all my subjects that the eyes were kept constantly fixed on the goal. If any eye-movements did occur, they could have been only slight and therefore confined to the middle portion of the scale, in which region the index-hand moved in a nearly lateral direction. Hence, the eyes being fixated upon the central goal with approximate constancy, movements of the eyes in the way of following the index-hand could have been only in a direction practically lateral, not up and down. If, however, we should suppose that larger eye-movements do occur, the effect would be as follows: On the 'A' side, where the index-hand is ascending, the eye-movement, due to the attempt to follow the course of the index-hand, would bring about, as Geiger has shown, a negative tendency in the displacement, and on the 'B' side a positive tendency. Now since a position of the index-hand on the 'A' side always corresponds in my experiments to a positive displacement, and a position on the 'B' side to a negative, the effects of following eye-movements would be simply to diminish the measured error in all cases and not increase the error on one side and decrease it on the other, as in Geiger and Wundt's method. Thus in any case, the results by my 'goal' method do not involve any factor due to eye-movement, except in so far as the separation of the 'A' results and the 'B' results may *possibly* be thereby less than it would otherwise be.

I have referred above (p. 29) to the different orders of succession of the velocities employed, first the order in which the velocities were increased from rate to rate, then the reverse of this procedure and finally the arrangement of the series whereby the extremes of speed became intermediate in position and the intermediate rates occurred at the beginning and at the end of the experimental hour. It might be expected that this succession of rates one after another, particularly in the

last-mentioned order in which the slowest rate is made to succeeed directly the fastest, would bring to light some influence of contrast. On careful scrutiny of the results, however, no essential difference appeared between the results for the same rates in the different orders. The method employed tended to eliminate contrast effects, because, determinations being made in groups of six with each rate, and requiring a number of revolutions for each judgment, the contrast effect, if present, would, unless very large, affect only the first judgments at a given rate, and hence would become inconsiderable in the average of the first judgment with the other five. The principal result of a contrast, if present, would be an increase in the mean variation.

Also, because of the order of succession of the four rates employed, the influence of practice does not prejudice the results of my experiments. By practice is here understood the effect of repetition and of adaptation to a constant condition. Since every rate was taken at each experimental hour, the averages given (see Table 1) each represent approximately the same amount of practice. However, the results for each rate were carefully gone over with respect to their time-order, and no variation of the results could be correlated with the progress of the experiment, *i. e.*, with the increasing repetitions.

In my discussion of Geiger's results an account was given of the two methods of observation which this experimenter found to be characteristic of the two types into which he differentiates his subjects, designating them the "naïve' and the 'reflecting' methods accordingly. I stated that Geiger attributed no exact significance to the terms by which he denoted these opposed types, but that he believed the two methods of observation thus designated often merged more or less into one another, so that the distinction which these terms indicated, was by no means a rigid one. Further, Geiger states that the terms are in themselves inadequate to classify the types in question; that, while the subject denoted as 'naïve' has in his general method of determination a tendency more or less aptly described by this term, it is yet unsatisfactory in its import, and he finds the term 'reflecting,' by which he des-

ignates the subject accustomed to the other habit of observation, equally inadequate. It is apparent that the terms are on the whole but faintly descriptive of the subjects' methods, and Geiger is frank to own their ineptitude. He therefore offers as alternative to this terminology the terms 'scale-type' and 'index-hand-type' of judgment—the scale-type corresponding to the 'reflecting' and the index-hand-type of judgment corresponding to the 'naïve.' For, it will be remembered, it is characteristic of the 'reflecting' type that the observer notes quickly and holds fast to the *position on the scale* at which the index-hand arrived, at the moment of entrance of the discrete impression; on the other hand it is characteristic of the subject of the 'naïve' type that he follows the index-hand with more or less of nonchalance until, without effort on his part, its position at the moment of the discrete impression seems established for him, whereupon he notes the corresponding position on the scale.

Undoubtedly Geiger has the right clue in assuming two types of observers, or rather two types of judgment, but his failure to isolate them sharply from one another may mean that he has caught only the one aspect of them which appears in his particular conditions of experiment.

My experiments had not proceeded far, when it became apparent that the method of making determinations was not constant; not that certain subjects followed exclusively a certain method throughout, while others observed according to an entirely different method, but that it was early seen that each subject inclined now to one way of proceeding and now to another way, so that two factors came prominently into consideration according to the methods in question. These factors were in the first place the perception of the *spatial* position of the discrete stimulus in relation to the alignment; *i. e.*, the angle between the goal and the position of the index-hand when the discrete stimulus occurred and secondly the perception of the *temporal* relation between the two stimuli; that is, between the moment of the alignment and the moment of the discrete stimulus, or *vice versa*. We have then two types or methods of procedure according as the spatial or the temporal

factor is the more prominent, and I have accordingly designated them the spatial and the temporal types.

It should be stated that the difference in the nature of the judgment depending upon whether the subject's determination of lack of simultaneity was really the estimate of the purely *temporal* interval between the discrete impression and the alignment, or of the spatial separation of the two corresponding positions of the index-hand, was brought to my attention entirely independently of Geiger's statement with regard to the two types of determination discriminated by him, and it did not occur to me at the time that what I called the spatial and temporal methods of determination could be correlated with the methods he described. That Geiger's 'naïve' and 'reflecting' types correspond to the two forms of judgment revealed in my experiments I hope to make clear in what follows. With Geiger, the 'naïve' type is the type of observer whose custom is to regard primarily the index-hand—to correlate the index-hand *per se* with the discrete stimulus. The observer of this type asks himslf *where* was the index-hand at the stroke of the sound; that is, he locates it not with regard primarily to the scale or marks on the dial, but rather by its angular position, probably by reference to the vertical and horizontal orientation. This does not mean that he explicitly recognizes the orientation or measures in degrees. But the spatial position of the hand at a given moment is noticed without the aid of the dial or scale.

On successive rotations of the index-hand, the observer continues to note the positions in the same way, and hence notices the angular distance between the position at which the index appears to be when the discrete stimulus occurs one time, and the position next time. When finally this deviation, which appears to him each time as the *error* in the preceding estimation, ceases to occur, the observer accepts the determination as final, and notes the scale position, if it have not previously obtruded itself upon him. It is seen that in this procedure the subject's estimate of the position of the points of his supposed error and of the consequent correction is expressed in spatial terms. The question of 'how far ahead?' or 'how far

behind?' is the subject's new determination with regard to the preceding apparently correct position is a question of the measure of the angular interval between the positions.

This spatial element in the determination is especially brought out through the method involved in the use of apparatus such as I employed, in which the subject is called upon to perform the active placing of the temporal position of the discrete stimulus at the moment at which the index-hand shall pass a certain definite point on the dial, setting out from various positions more or less remote, upon either side of the position to be attained. It was found that in first setting out certain subjects more readily took account of the spatial than of the temporal interval between the index-position at which the discrete stimulus was given them (75°, 60°, 45° 'A' or 'B') and the position (at 0°) to which they had to shift it. The spatial separation was for them the easier datum to reckon with in this case. Where the interval was wide, as always in the beginning of each experiment, it was the more comfortable way of regarding the separation of the two positions. As the two termini stood out pictorially before him, the subject quite unconcernedly noted them together. As the method involved less the feeling of strain upon the attention than would have been occasioned through the attempt to connect up in a single conceptual act the temporal relation of the two impressions, at least for a certain type of subject, such a subject, upon setting out, fell naturally into this habit of observing. It is because of this characteristic leisure-attitude of the subject in making judgments according to the method which obtains in spatial estimates of the lack of simultaneity that I am led to identify the observer of this type with the one whom Geiger calls 'naïve' or of the 'index-hand' type.

In the second method of observation the time-element is predominant. With this type the influence of the sense of the spatial relation of the two positions: *i. e.*, the position which the index-hand first appears to occupy at the occurence of the discrete stimulus and the position of zero at which the subject is to place the latter, is apparently eliminated. The subject makes of it a time-judgment, and the experiment becomes

substantially an attempt to reduce an initially large time-interval to zero.

In experiments like Geiger's the 'reflecting' subject watches a scale-division to notice if the appearance of the index-hand at it is simultaneous with the appearance of the complicating stimulus; and if it is not, he tries other scale divisions, to one side or the other of the first. From this description it is obvious that the temporal form of judgment as it appears in my experiments is justly to be correlated with the 'reflecting' or 'scale-division' type of judgment as shown by Geiger. Now the temporal method is quicker than the spatial; that is the subject reaches his final determination after fewer revolutions of the index-hand than are required for the subject whose judgments are of the spatial type. The reason is undoubtedly this: in the temporal form of judgment the subject is dealing successively with two sharply limited, clear-cut impressions; the momentary discrete impression and the momentary impression of the scale-division which the subject selects to note as the index-hand sweeps past it; each is brief and clearly defined. But in the method of spatial determination the subject is necessarily occupied with both details at once, and moreover, perceives the angular position of the index-hand indistinctly on account of its being in motion and traversing the very angle it marks off. Hence the individual judgments are less clear by this method, and more revolutions are needed before the subject arrives at a satisfactory determination, just as Geiger found to be the case with the 'naïve' type as compared with the 'reflecting'. In this agreement in difference of time required for making a judgment, there is then further ground for correlating my temporal and spatial types with the 'reflecting' and 'naïve' types respectively of Geiger.

It must not be understood that the two types which I have designated 'spatial' and 'temporal' are to be ascribed to individuals adhering rigidly to one or other of them: that one observes exclusively with the assistance of spatial data and another type exclusively with respect to temporal data, but rather that they are two methods which coöperate in most cases. It seems probable therefore that each subject is swayed

more or less by the influences of both methods, and that the type can be determined only as the predominance of one of the two methods. As said above, it would seem that in my form of apparatus, the obviously wide spatial interval between the place of the stimulus when setting out and the goal to which it is to be brought renders the spatial method of observation the more convenient at the beginning of a determination. To compute the interval in terms of the spatial perception of its termini is instinctively felt to be the more economical procedure. When, however, the interval becomes narrow (as when the subject has shifted the stimulus toward the goal) and may not be readily computed because of its narrowness the tendency to rely upon the relative *temporal* position of the two stimuli (discrete impression and alignment of index-hand with central goal) doubtless comes more strongly to the fore. And further, precisely in consequence of the obliteration of the appreciable spatial interval—the interval over which the index-hand is required to move—the method becomes *ipso facto* the method of 'scale-division' observation, *i. e.*, in my terminology, the temporal method. The data are reduced to two: alignment of the index-hand with the scale-division or the goal, and the discrete stimulus,—flash or sound—which is to be correlated with it.

Of course it is not impossible that the subject of the temporal type should sometimes adhere to this method even in the beginning of an experiment, feeling the temporal relation of the stimulus to the goal when first setting out; but even in the temporal type of subject it is impossible to say how great a part in general the beginning spatial tendency plays in the ultimate determinations.

The two factors of spatial and temporal judgment were introspectively observed by Subjects I and IV, who were convinced (Subject IV was myself) of the validity and importance of the distinction. Experiments were later undertaken with Subjects I and IV, in which the attempt was made to isolate these two factors by having the subject observe at one time the spatial, at another the temporal relation of the stimuli but these experiments were abandoned because of the great

difficulty of adhering consistently throughout to either of the two methods. It was not possible to isolate them adequately, and the devising of methods by which this isolation shall be suitably made belongs to another and lengthy experiment.

It is sufficiently clear, however, that the two factors are present, and my own attempts at an analysis of the elements involved lead me to the conviction that their importance demands a new development of methods and apparatus for adequate experiment in the future upon this problem. Here, I purpose to show that the phenomena of positive and negative displacements depends upon these two types of judgment, and that the heretofore adduced theories are correspondingly inadequate.

Geiger says "The results of the complication experiment appear to contradict normal reason:" for "one would naturally expect that the sound would always be united with a later scale-division than that with which it is actually simultaneous" and the explanation that follows assumes the onus of allaying our disappointment in this expectation. But Geiger neglects one important consideration. Were the assumption true that we are here dealing with a pure time-judgment, then it would undoubtedly contradict the presuppositions of normal experience to record that the subject, instead of selecting a later position than the actual one, should in general, actually select a position on the scale earlier than the real one. But we are not here dealing with a pure time-relation; in other words, the complication experiment has not been, in truth, the problem which all this while it has been tacitly assumed to be, namely, the determination of the *temporal* relation of a discrete impression to a series of similar impressions, into the course of which it enters at regular intervals, but the experiment is seen to become, on analysis, a determination of the *spatial* as well as of the temporal relation of the stimuli in question. Doubtless this prevailing misconception of the essential conditions determining the judgment in complication work is responsible for the wide disparity in the explanations which different observers offer to account for the phenomena, and its recognition reconciles in a measure the fundamental variances amongst them.

Let us come now to the explanation of the displacements, based on the difference in the types of observers, which my method of experiment brings out. In agreement with Geiger and the other German experimenters, I regard the positive errors, *i. e.*, the displacements to a position later than the actual one, to be the more natural errors. On the other hand, in opposition to these theorizers, I hold that these errors would actually be found prevailing were the displacements in reality only time-displacements; *i. e.*, were the measured error an error exclusively in estimation of the temporal relations of the stimuli in question; were the problem simply one of time-relation as psychologists have heretofore implicitly assumed it to be. For even in the experiments which do not exclude the presence of the spatial factor, the positive displacements are the prevailing ones with subjects who are predominantly of the temporal type. For the subject who observes strictly the temporal relation of the stimuli the discrete stimulus is the preëminent one, although it does not necessarily receive the most attention. Besides this, the discrete impression is the more sharply delineated and the exact instant of its entrance is noted with less expenditure to the subject than the momentary position of the index-hand corresponding to it. Hence the discrete impression becomes the determinate term into relation with which the indeterminate impression, namely, the corresponding scale-division must be brought.

unless perception of dial position delayed.

The problem then reduces to a question of the just imperceptible time-interval. The subject is to catch the position of the index-hand simultaneous with the discrete stimulus. Adverting from one to the other with the utmost possible promptness; he will normally select a position necessarily somewhat behind, that is, somewhat later than the position to which the sound (or flash) actually corresponds. For, a certain time, however brief, being lost in the transition necessarily, in this time, minute though it be, the index-hand will have traveled over a greater or less interval, and consequently the judgment will fasten upon a position later than the true one. However, Angell and Pierce are wrong in asserting that the subject does not experience the simultaneity of the two impressions, but

that he is satisfied with his judgment when he has succeeded in approximating a position of simultaneity as ascertained by the feeling of a very brief interval between the auditory and the visual impression. The subject, being acquainted with the object of the experiment, necessarily understands that the whole problem turns upon his appreciation of simultaneity, and that he has satisfied the essential conditions of the experiment only when he has succeeded in determining the position on the scale at which the index-hand seems to him to arrive simultaneously with the occurrence of the discrete stimulus. How else could the subject assume the occurrence of the two impressions to be simultaneous than by the subjective consciousness of simultaneity? What criterion has he for determining simultaneity other than the appearance of simultaneity? Now, in a determination made in conformity with the procedure of the temporal type of observer, the place at which the index-hand appears to be simultaneous with the discrete impression will be found to be, as a rule, *after*, *i. e.*, farther along the scale (in the sense of the direction of the rotation of the index-hand) than the real position of simultaneity, and for the reason that the discrete impression is at the final judgment the primary one, and a certain time-interval between it and the arrival of the index-hand at a certain point on the dial will be unperceived; hence the arrival at this point will be apprehended as simultaneous with the discrete impression.

The procedure just described is normal in determinations in which the prevailing tendency is to the temporal form of judgment; but it is of course probable that the imperceptible time-interval will occasionally be reduced to zero, in which case there will be no displacement; or that the discrete stimulus will sometimes be taken as secondary and the arrival of the index-hand at the point on the dial as primary, in which case a negative error will result. We have here then, an explanation of the occasional so-called correct judgments, though in truth a determination which actually corresponds to the real position of simultaneity is no more correct subjectively than a determination which gives the maximum displacement; for in each case there is subjective consciousness of simultaneity.

Wundt is therefore unwarranted in alleging the presence of these occasional random correct judgments as proof of the ability to experience two impressions as simultaneous, for as an experience of simultaneity it is no more trustworthy than the experience of simultaneity in those cases in which the judgment actually shows a considerable displacement.

As just said, occasional negative displacements are accounted for by the subject's inversion of the usual time-order. But what is the true account of the general prevalence of negative errors, such as shown by all the experimenters up to the present time, with the single exception of Angell and Pierce, whose results showed a prevailing tendency to positive displacements? In my account of the opposed direction of displacements I am led from the results of observation and introspection to rely upon the factor of the opposed method of observation characteristic of the spatial type of observer and I may immediately summon to support my argument the fact just cited, that Angell and Pierce, who throughout their experiments adopted the method of determination recognized by Geiger as belonging to the 'reflecting' type (already identified with the temporal type of my classification), and who were the only investigators to observe exclusively according to this method, were also the only observers who obtained throughout a preponderance of positive displacements; agreeing with the quality of the displacements Geiger found to obtain with those of his subjects who observed according to the corresponding method.

What then was the method adopted by the other investigators? Geiger, who made inquiry of two of those preceding him and even conducted a few experiments upon them in the hope of finding them of the 'naive' (or spatial) type and so confirming his observation that the 'naïve' observers show negative results, was disappointed, for at least one of the observers (a subject of Pflaum's) proved of the 'reflecting' type. Geiger therefore was led to attribute the discrepancy to the apparatus employed and concluded that the pendulum apparatus rendered difficult the observation according to the strict 'reflecting' method, owing to the difficulty of steadily fixating a single scale-division, due to the variability in the movement of the

index-hand and the resulting obscuration of the scale-divisions in its course.

If the increasing strain of attention described by Wundt is responsible for the occurrence of negative displacements, it would seem to be a more potent factor in determinations of the temporal type; in which the increasing strain of attention may occasion an anticipation of the alignment. But this anticipation, as we have said before, accounts only for occasional negative displacements. But what of the negative displacements occurring in the large majority of cases? To explain these errors which are due, as we have stated, to the spatial form of judgment, it is necessary to invoke the fact insisted upon by Wundt and Geiger, that the interval in the appearance of the two stimuli is a relative, and not, as von Tschisch thought, an absolute one. It is a question of the relative time of apperception, as Wundt terms it, of the auditory and visual stimuli. Now those subjects who identify the discrete stimulus with a position of the index-hand spatially separated from the position on the scale at which index-hand and momentary stimulus are simultaneous, the attention being fixed more upon the discrete stimulus, are led by the sense of its spatial relation to the rotation of the index-hand, that is, by the feeling that the discrete stimulus somehow enters from a position somewhere in the direction from which the index-hand approaches, to identify the discrete stimulus with a position earlier than the actual one in the course of the index-hand. That is to say; to the observer of the spatial type, the traction upon the attention exerted by the feeling of the direction from which the spatially identified discrete stimulus appears to enter—the direction of ntrance of the index-hand with which it is allied in space—causes the subject to displace it in the direction of its apparent entrance, thus uniting it with an earlier position of the index-hand than the correct one and giving as a result a negative error. This is the element in determining the judgment which I called factor Number 2, and which has been up to the present undisposed of.

Obviously, the greater the number of repetitions of the st mli, *i. e.*, the greater the number of preliminary rotations of the

index-hand, the greater the effect of the traction upon the attention by the discrete stimulus due to the influence of the sense of its direction of entrance; the discrete stimulus, as just explained, being identified in the mind of the subject with the direction of movement of the index-hand and having its spatial significance augmented through this association with this movement in space. In this way may be explained the experiments of Geiger in which, observing the error obtaining with each rotation from first to last, the position of apparent simultaneity was found to recede progressively, *i. e.*, to shift with each rotation to a position farther removed in the direction from which the index-hand approaches. This assumption likewise affords an explanation of the tendency to greater negative errors with the slower speeds, as shown by the experiments of the Leipsic investigators; for naturally, the slower the rotation of the index-hand, the more potent upon the attention is the influence of the traction of the discrete stimulus in the direction opposed to that of the rotation of the index-hand, and consequently the wider the displacement in the negative direction.

With subjects requiring a large number of preliminary rotations, the number of such preliminary rotations decreases with practice, hence there is here another factor explaining the greater positivity upon practice, as found by Angell and Pierce and by Geiger. For, the greater the number of revolutions of the index-hand, the more effectual is the influence, as just said, of the sense of traction in the direction from which the complicating stimulus seems to enter; that is, the greater the negativity. With practice the amount of the negative error decreases as the number of rotations necessary to a determination decreases, independently of the change of the subject towards the temporal type. So upon my hypothesis of a space judgment to account for the presence of the negative error is afforded the solution of the problem of the influence of practice in decreasing the negativity of the results.

I have explained Angell and Pierce's positive results as due to their usage of the temporal type of observation. But other influences tending to bring about positive errors in their

results are these: first, they did not take account of the lag in the complicating stimulus due to the interval of time required for the hammer to act and for the transmission of the sound waves from it to the subject's ear; and secondly, the conditions of their experiments were especially favorable to the forward, over-reaching movements of the eyes, caused by the impetus received from the downward course of the index-hand as it enters upon the arc constituting the scale; the latter, as explained before, being placed below the center of revolution and in this regard being unlike the position of the scale with most of the other experimenters. The fact that Angell and Pierce noted no influence of speed is also due to their method of experiment, for speed is of little or no moment in observations of the temporal type, since here it is a question only of the time between a given stimulus and alignment, and the distance traversed by the index-hand in that time is of little consequence. In the spatial judgment, on the other hand, this distance is noted and becomes influential.

The effects of the temporal and spatial methods on the relative distribution of attention to the discrete impression and to the alignment, are yet to be considered. In the temporal form of judgment the attention is maximally on the alignment of the index-hand with the selected point on the dial, for the noting of that is the task to be executed immediately upon the occurrence of the discrete impression. In the spatial or 'naïve' form of judgment the attention is more evenly distributed between the two factors, but is greater if anything to the discrete stimulus, since it is that which picks out a position of the index-hand. Hence it would be expected that voluntary direction of the attention in a way characteristic of the one or the other of these forms of judgment would incline the judgment towards that form, as has been actually found to be the case by Stevens, Titchener and others.

An important point requiring consideration is the hitherto unrecognized question of the possible influence in the experiments earlier conducted of a factor analogous to my 'A' and 'B' directions of setting out and to Klemm's directions of approach from the two sides. We have seen the very striking

effect of the position of setting out upon the resulting judgment, as shown in my experiments (see Tables 3 and 4). Now, the influences of the positions of setting out are here equally balanced on the two sides of the goal and cancel each other in the averages. But what of the influence of the position of setting out upon the results of Wundt, von Tschisch, Pflaum, Angell and Pierce, and Geiger? For, if such an influence were present and not taken into account, operating at one time to displace the judged position in one direction, at another perhaps in the other direction, the consequence of the neglect of so considerable a factor is to render uncertain the significance of the nominal results. Obviously there was such a factor present, for since a determination was not made until after several rotations, particularly in the case of the 'naïve'(spatial) observer, it is clear that corrections were made and there was therefore a point of setting out. Now it is not to be supposed that the subject had always a correction of equal magnitude to make, that he had always to advance or withdraw the position of his determination (depending on whether it appeared to him in front of or behind the new position) by the same amount with every final judgment. Nor is it to be assumed that the position of his tentative first judgment was always upon the same side of the final position in every determination. Yet in the case of some particular subject and rate one or both of these conditions might be realized. Clearly then there was in the earlier investigations the factor corresponding to my 'A' and 'B' factor and experiments conducted without regard to it fail to take account of one of the most potent of the controllable factors occurring in the complication experiment.

To summarize briefly the chief points which my investigation brings out: it shows first the fact that contrary to the implication of Wundt, a displacement occurs where the discrete and the serial stimuli belong to the same sense realm, and even to contiguous portions of the same sense organ, as proved by those experiments in which I employed a momentary light-stimulus instead of the disparate impression employed by preceding experimenters. In the second place, it brings out the difference in the determination depending upon the side ('A'

or 'B'), of the scale from which the subject set out, and the distance of the position of setting out from the terminal position.

And finally, I have shown how the two types of observation found by Geiger, the effects of attention to the discrete impression or to the serial impression, the occurrence of positive displacements predominantly with some subjects and of negative with others, and the positiving effects of 'practice,' are all brought into agreement and made intelligible by the discovery and analysis of the distinction between the 'spatial' and the 'temporal' forms of judgment.